KONINKLIJKE AKADEMIE VAN WETENSCHAPPEN TE AMSTERDAM.

PROCEEDINGS OF THE MEETING

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Zoology. — "*On gastrulation and the covering of the yolk in the teleostean egg.*" By Dr. J. BOEKE. (Communicated by Prof. A. A. W. HUBRECHT).

(Communicated in the meeting of January 26, 1907).

1. Generally the process of gastrulation in teleosts is described by the greater part of the embryologists as a folding in of the margin of the blastoderm and the forming, partly by this process of folding and partly by delamination, of a mass of cells that contains the elements both of the chorda and mesoderm and of the entoderm. Only WACLAW BERENT, M. v. KOWALEWSKI (in his paper of 1885), F. B. SUMNER and myself have described a more or less independent origin of mesoderm and chorda on one side and the entoderm on the other side. SUMNER called the mass of cells lying at the posterior end of the embryo, from which the entoderm originates, prostomal thickening; I kept the same name for them and regarded these cells as being derived from the periblast.

The large pelagic eggs of Muraenoids, which I could collect in large quantities at Naples, offer an extraordinarily good object for the study of these processes, much better than the eggs of Salmonides, studied chiefly by French and German authors [1]). The formation of chorda and mesodermic plates out of the folded portion of the blastoderm, and of the entoderm out of the "prostomal thickening", the mass of cells that lie at the hind-end of the embryo and are connected with the superficial layer and with the periblast, is clearly to be seen from the beginning of the formation of the embryo until the closure of the yolk-blastopore (confirmed by SUMNER in his paper of 1904) and after a renewed careful study of these eggs [2]) I can only confirm entirely and in full the conclusions arrived at in my former paper [3]) and the observations described there at some length.

But in accordance with the new and better definition of gastrula-

[1]) Neither HENNEGUY, nor KOPSCH or JABLONOWSKI, to take a few examples, did see anything of these differentiations. SUMNER gives however of Salvelinus very clear figures and descriptions. (Arch. f. Entwickelungsmech. Bd 17. 1903).

[2]) During the last 2 or 3 years Muraenoid-eggs seemed to have disappeared entirely from the Gulf of Naples. Now (summer 1906) I found them again in sufficient quantities. When comparing the different eggs with each other, it seemed to me that they belong to a still larger number of different species than I concluded in my former paper (9), and that there must be distinguished at least 10 different species of Muraenoid eggs in the Gulf of Naples. Dr. SANZO at Messina came to the same conclusion.

[3]) PETRUS CAMPER, Vol. 2, page 135—210 1902.

tion in vertebrates, given by HUBRECHT and KEIBEL and confirmed by a number of other embryologists, this process in the teleostean egg too must be revised and more sharply defined.

In my former paper I was led to divide the process of gastrulation into two "phases", one by which the gut-entoderm is formed and one by which chorda and mesoderm are differentiated. But now I think the line must be drawn still sharper and the second phase must be separated entirely from the process of gastrulation sensu strictiori.

According to the definition given by HUBRECHT gastrulation is a process by which a gut-entoderm is differentiated from an ectodermic layer, and thus the germ consists of two distinct layers. The process of formation of chorda and mesodermic plates, which follows directly on the process of gastrulation proper (notogenese HUBRECHT) is a secondary complication of the process, characteristic of the vertebrate embryo.

The most primitive mode of formation of the entoderm, according to HUBRECHT, is by delamination and not by invagination. But after all it is chiefly the outcome, the formation of the two germ-layers, that is of interest. As soon as these two layers are formed and may be distinctly separated from each other, the process of gastrulation is finished.

This is for example in amphioxus already the case at that stage of development, in which the gastrula is cap-shaped, the two layers (ectoderm and entoderm) are lying close against each other, the segmentation-cavity has disappeared, but the blastopore still extends over the entire breadth of the original blastula-vesicle. All the following processes until the closure of the blastopore ("Rückenmund" of HUBRECHT) are notogenesis and lead to the formation of the back (chorda) and of the mesodermic plates and to the closure of the gastrula-mouth.

When we study again the teleostean gastrulation-process from this point of view, we come to the conclusion, that in this case the process of gastrulation is ended as soon as the prostomal thickening has been formed, viz. at the beginning of the covering of the yolk. At that moment the "Anlage" of the entoderm is clearly differentiated, and the ectodermal cells begin to invaginate to form the chorda and mesodermic plates; the concentration of the cells towards the median line begins, the long and slender embryo is formed out of the broad and short embryonic shield. The blastula-cavity, in the cases in which it is developed, has disappeared as such; all the following processes, the longitudinal growth of the embryo, the covering of the yolk by the blastoderm ring, the closure of the yolk blastopore, belong to the notogenesis and we are no more entitled to reckon these processes

to gastrulation proper as we are to do that of the covering of the yolk by the entoderm in sauropsids. During this longitudinal growth of the embryo new cells are produced by the prostomal thickening and pushed inwards to form the entoderm, but this may not be called gastrulation any more. The period of development, during which the yolk is being covered by the blastodermring, differs much in different embryos. In muraenoids at the time the yolk-blastopore is closed the embryo possesses from 5 to 10 pairs of primitive segments; the issuing larvae possess 58 to 75 segments. In salmonidae at the closure of the yolk-blastopore of the 57 to 60 segments 18 to 28 are differentiated. The other organs too are developed to a greater or lesser degree. To use the term gastrulation for the processes during this whole period of development leads us into difficulties.

The first question we have to answer, when we study closer the process of gastrulation in teleosts, is: at what time does the process of gastrulation begin in the large meroblastic eggs?

Recently Brachet [1]) has called attention to a process, which he calls "clivage gastruléen", and which he describes for the eggs of Rana fusca as the formation of a circular groove at the base of the segmentation-cavity around the yolk-mass, before there is to be seen a trace of a blastopore (Rusconic groove) at the outside of the egg: "immédiatement [2]) avant que la gastrulation ne commence, la cavité de segmentation, sphérique ou à peu près, occupe l'hémisphère supérieur de l'oeuf (de Rana fusca).... Bientôt, sur tout le pourtour du plancher de la cavité de segmentation, une fente se produit par clivage; cette fente 's enfonce entre les cellules de la zône marginale et les divise en deux couches: l'une, superficielle, prolonge directement la voûte de la cavité de segmentation, mais est formé par des cellules plus volumineuses et plus claires qu' au pôle supérieur; l'autre, profonde, fait corps avec les éléments du plancher. C'est ce clivage, que j'ai appelé "clivage gastruléen", c'est lui, qui caractérise la première phase de la gastrulation, parce qu'il amène, en dessous de l'équateur de l'oeuf, la formation d'un feuillet enveloppant et d'une masse cellulaire enveloppée, d'un ectoblaste et d'un endoblaste." And farther on: "lorsque ce clivage est achevé, il est clair, qu'a sa limite inférieure, l'ectoblaste et l'endoblaste se continuent l'un dans l'autre, comme le faisaient antérieurement la voûte et le plancher de la cavité de segmentation."

This line of continuity Brachet calls "blastopore virtuel"; after a short time this virtual blastopore is converted into a real blastopore

[1]) Archives de Biologie Tome 19 1902 and Anatom. Anzeiger. Bd. 27 1905.
[2]) Anat. Anzeiger Bd. 27, p. 215.

by the formation of the groove that leads to the formation of the archenteric cavity. This groove is formed by delamination; until now there is no trace of invagination. This begins in what BRACHET calls the second phase of the gastrulation process, which leads to the formation of the archenteric cavity in its entire width, and is synchronic with the process of notogenesis, of the formation of the back of the embryo; "quand les lèvres blastoporales se soulèvent, quand de virtuelles elles deviennent réelles, c'est que le blastopore va commencer à se fermer, c'est que le dos de l'embryon va commencer à se former" (l.c. 1902, p. 225).

BRACHET is right here. Also there, where be draws a sharp line between the entirely embryogenic blastoporus of the holoblastic eggs and the blastoporus of the meroblastic eggs with a large amount of yolk, which is divided into two parts, an embryogenic blastoporus and a yolk-blastoporus.

But when he reckons these processes, which occur in the selachian and teleostean egg during the covering of the large mass of yolk and the closure of the blastopore, still to gastrulation, when he calls the entire process of covering of the yolk "clivage gastruléen", and calls the whole blastoderm ring "blastopore virtuel", he goes too far, and forgets the significance of the phenomena, occurring at the end of segmentation and during the formation of the periblast.

For the answer to the question, at what time does the gastrulation in the teleostean egg begin, his analysis of the phenomena of this process in the amphibian egg is extremely interesting.

The segmentation of the teleostean eggs is not regular during all its phases. When we combine the very accurate observations of KOPSCH on this account, we see, that in the segmenting blastoderm at a definite moment, about that of the 10th division of the embryonic cells, there occurs an important alteration.

Until the end of the 10th cell-division (in Belone) the different cells divide wholly synchronic; in Torpedo RÜCKERT found synchronism until the 9th division. By the tenth division the yolk-sac entoblast is formed (in Gobius, Crenilabrus, Belone), the two nuclei of the marginal segments, resulting from this division, remaining in the undivided protoplasm; where this does not occur at the tenth division the deviation is very small (in Cristiceps argentatus it partly begins at the 9th division, in Trutta fario at the eleventh division). Synchronically with the differentiation of yolk-sac entoblast the superficial layer ("Deckschicht") is differentiated. At the end of the 10th division all at once the blastoderm alters its form: it gets higher, more hill-shaped and the diameter is lessened; the mass of cells

concentrates, the superficial layer is still more clearly visible as a definite enveloping layer of cells. It is just the synchronic differentiation of the superficial layer, which shuts off the blastoderm from the surrounding medium and is the only way by which the developing cells may get the oxygenium from the perivitelline fluid, on one side, and of the periblast, by means of which the blastoderm is nourished by the yolk, on the other side, which seems to me to be important; by this synchronic differentiation a new phase in the developmental process is initiated, and the series of changes have begun that lead to gastrulation.

Very soon the blastoderm-disc flattens, at first only because the superficial layer contracts a little, and the blastoderm sinks a little into the yolk-sphere (fig. 8) but after that because the blastodisc itself spreads out, flattens (fig. 9). The cells come closer together, and soon the unilateral thickening that forms the first outwardly recognisable beginning of the building of the embryo, becomes visible.

During these changes it is of no account whether a blastula-cavity is formed, or not. As I have described elsewhere, in different muraenoids during this stage a distinct blastula-cavity is formed, which may be seen in the living egg. Afterwards follows the flattening of the blastodisc and the disappearance of the cavity as such. The closer study of young stages of the eggs of muraena N°. 7 [1]) showed me however, that in these eggs no blastula-cavity is formed, and that in this case the blastoderm, that takes just the same conical shape as the hollow blastoderm in the other muraenoid eggs, remains solid and is built up of a mass of loosely arranged cells. The further development is the same as in the other series (c.f. fig. 1—3 on plate 1).

This flattening of the blastodisc, following on the stage just described, coinciding with the concentration of the cells of the blastoderm towards the side where in later stages the embryo is formed, and coming before the invagination (and partial delamination) of the blastoderm cells, that leads to the formation of the chorda and the mesodermic plates, is already a part of the gastrulation process and must be compared with the "clivage gastruléen" of the amphibian egg.

Immediately on this "clivage gastruléen" follows the formation of the prostomal thickening (that is the "blastopore réel" of BRACHET), there where the superficial layer or pavement layer is connected with the periblast, out of the surperficial cells of the periblast [2]) (c.f.

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[1]) Comp. PETRUS CAMPER, Vol. II p. 150.

[2]) SUMNER (l. c. page 145) saw evidences for this mode of origin in the egg of Salvelinus, but not in that of Noturus or Schilbeodes. On these two forms I can-

fig. 4, 5 and 6 on plate 1). It seems probable, that at least in some cases entodermcells are formed by delamination from the periblast at some distance from the surface in front of the prostomal thickening (fig. 5*e*). So here, as in many vertebrates, the entoderm is formed by delamination. At the moment of the differentiation of the prostomal thickening (figs. 2, 4), there is still no trace of the invagination of the mesodermcells, only a thickening of the mass of cells lying just overhead of the cells of the prostomal thickening. Immediately afterwards however a distinct differentiation of the mesoderm becomes visible. At that stage the notogenesis begins and the gastrulation process is finished. The prostomal thickening is the ventral lip of the "blastopore réel" of the Amphibian egg. For the developmental processes following on this stage I can contain myself with referring to my former paper. That here only a small, not very prominent tail-knob is formed and no far-reaching projecting tail-folds appear, as in the selachean embryo, is caused by the relation of the pavement-layer to the blastoderm and the periblast, which influence the development of teleostean egg ("développement massif" of HENNEGUY).

2. To determine the direction of growth of the blastodermring during the covering of the yolk, I used in my former paper the oil-drops in the yolk of the muraenoid eggs as a point of orientation, on the contention that these oildrops maintain (in the muraenoid egg) a constant position in the yolk. On this basis I constructed a scheme of the mode of growth of the blastoderm in the yolk. [1]) Both SUMNER and KOPSCH rejected this contention and the scheme, SUMNER because of the fact, that by inverting the egg of *Fundulus heteroclitus* in a compress, the oil-drops may be caused to rise through the yolk and assume a position antipodal to their original one, which shows, that here the oil-drop may not be regarded as a constant point of orientation in the egg. In this SUMNER is perfectly right. Not only in Fundulus, but in several marine pelagic eggs too the oil-drops may be seen travelling through the yolk by converting the egg or bringing the young larva (in some species) in an abnormal position. In the muraenoid egg however the case is entirely different. Here the structure of the periblast and of the

not judge, but I will only mention here, that the figures, drawn by the author, are taken of much too late stages of development, to be convincing. And after all, where the blastodermcells are so much alike, as is the case in most teleostean eggs, one positive result in a favourable case as is offered in the muraenoid egg, is more convincing than several negative results in less favourable eggs.

[1]) l. c. page 142.

yolk-mass, which I described at full length in my former paper, completely checks the displacement of the oil-drops. This is to be concluded already from the behaviour of the normal egg. So in the eggs of Muraena No. 4 a large number of rather large oil-drops are lying at about equal distances from each other at the surface of the yolk-mass. During the entire process of covering of the yolk, the distance of these oil-drops remains the same, they maintain their relative position absolutely, and only during the slight disfigurement of the yolk-sphere, caused by the contraction of the blastodermring during the circumgrowth of the yolk (fig. 4 on plate 2) the position of the oil-drops is changed a little, only to become the same as before, after the yolk has regained its spherical form. When these oil-drops were lying loose in the yolk or in the periblast, they would have crowded together at the upper pole of the egg, or at least their relative position would have undergone a change during the covering of the yolk. Only when the yolk-mass in the developing embryo becomes pear-shaped and very much elongated (l.c. plate 2, fig. 6, 7), the oildrops of course change their position. Even then, however, they remain scattered through the yolk.

Experiments also show the constant fixed position of the oil-drops in the muraenoid eggs. When we transfix the egg-capsule carefully with a fine needle, it is possible to lift one of the oil-drops or a small portion of the peripheral yolk out of the egg. The other oil-drops retain their normal position, and in most cases such eggs develop normally and give rise to normal embryos. When we operate very carefully under a low-power dissecting-microscope, it is possible to leave the oil-drop connected with the periblast by means of a thin protoplasmatic thread. When we do this in a very early stage of development, at the beginning of the gastrulation-process, we see that this oil-drop, which surely may be regarded as a fixed point on the surface of the egg, retains its position in relation to the other oil-drops, until it is cut off from the periblast by the growing blastodermring. In fig. 2*a*, 2*b*, 2*c* and 2*d* on plate 2 I have drawn from life several stages of this process in an egg of Muraena No. 1. During my stay at the Stazione Zoologica at Naples, in August and September 1906, I performed several of these experiments with different muraenoid eggs. They all led to the same result, and confirmed my former statements. And so I believe that my contention was right and that the scheme I figured is a true representation of the facts. Of course only in a general sense, for there are many individual variations (so for example the case figured in fig. 3 on plate 2). And after all, when we compare this scheme with that

given by KOPSCH for the trout, we see that they do not differ so very much, and that the displacement of the hinder end of the embryo is almost the same. In the text of my former paper however I expressed myself rather ambiguously, and brought my view into a too close contact with that expressed by OELLACHER. The figures however show that my scheme differs rather much from that of OELLACHER.

But I differ from KOPSCH in his supposition that the h e a d-end of the developing embryo is a fixed point on the periphery of the egg. I find myself here quite in harmony with SUMNER, who draws from the large series of his extremely careful and exact experiments the conclusion, that "the head end also grows, or at least moves, forward, though to a much smaller extent" (l. c. page 115), and says: "I regard it as highly probable (see Exp. 1, 3, 34, 35, 36 and Fig. 32) that the primary head end grows — or is pushed — forward from an original position on the margin" (l. c. page 139).

From different experiments of the author we may draw the conclusion, that in many cases this forward growth of the head-end is rather extensive (exp. No. 6, 10, 11 (partly), 26, 35, fig. 10), and experiment N°. 6 (table VIII) among others shows, that under circumstances the direction of growth may be entirely reversed, so that the tail-knob of the embryo remains at the same place, and the head-end bends round the surface of the yolk.

KOPSCH too, in his paper: "Ueber die morphologische Bedeutung des Keimhautrandes und die Embryobildung bei der Forelle", describes an experiment with simular results in the trout.

So it is not unreasonable to suppose, that in the spherical egg of the Muraenoids during the covering of the yolk the head end of the embryo is moving forward, and to a certain extent follows the growing blastodermring, which is the case chiefly during the later stages of the covering of the yolkmass, as I showed in my scheme. During the first stages of development it is chiefly the tail-end of the embryo which travels backwards, (see the scheme in my former paper and fig. 1—3 or plate 2), and KOPSCH is right to locate here the centre of growth of the embryo.

The conclusion of SUMNER, that for some time prior to the closure of the blastopore, the ventral lip of the latter (former anterior margin of the blastoderm) travels much faster than the dorsal lip (l. c. p. 115) is quite in harmony with my results for the muraenoid egg described in my former paper. [1])

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[1]) PETRUS CAMPER, l. c. p. 196.

3. At the end of the covering of the yolk, at the closure of the blastopore, KUPFFER's vesicle is formed after the manner described at length in my former paper. By SWAEN and BRACHET [2]) and by SUMNER the narrow passage connecting this vesicle with the exterior, through the closing blastopore, is regarded as representing the neurenteric canal. I do not think they are in the right here. KUPFFER's vesicle is a ventral formation. Dorsally it is separated from the cells of the medulla by the cells of the prostomal thickening and the pavement layer. An open canalis neurentericus is not found even in these forms. KUPFFER himself called the vesicle allantois. HUBRECHT followed him in this. In my former paper I compared the vesicle with the allantois of amniota on physiological grounds, and I think it is a very good thought of HUBRECHT to take up the old name of KUPFFER and compare the vesicle with the allantois on morphological grounds.

DESCRIPTION OF FIGURES ON PLATE 1 AND 2.

Plate 1.

Figg. 1—4. Median sections through eggs of Muraena N°. 1 on different stages of gastrulation. In fig. 3 gastrulation is finished and notogenesis is begun. In fig. 2 the structure of the yolk is drawn. Enlargement 40 times. Fig. 4*a*, 5 and 6 give median sections through the developing prostomal thickening and adjoining parts, seen under a higher power.

Figg. 7—9. The flattening of the blastodisc at the beginning of gastrulation in eggs of Muraena N°. 7. Enlargement 40 times.

Plate 2.

All the figures on this plate are drawn from life as accurately as possible.

Fig. 1*a*—1*e*. Covering of the yolk in an egg of Muraena N°. 1.

Fig. 2*a*—2*d*. Covering of the yolk and closure of the blastopore in an egg of Muraena N°. 1. By means of a fine needle one of the oil-drops is nearly severed from the surface of the yolk, remaining connected with the periblast only by means of a thin protoplasmatic thread. In fig. 2*c* this oil-drop is cut off from the surface of the egg by the travelling blastodermring and is lying close against the egg-capsule EK. In fig. 2*d* (closure of the blastopore) this oil-drop is no more drawn in the figure.

Fig. 3. Unusually fargoing dislocation of the hinder end of an embryo during the covering of the yoik. The head end lies approximately at the former centre of the blastodisc.

Fig. 4. Compression of the yolk-sphere by the growing blastodermring in an egg of Muraena N°. 4. The oil-drops only temporarily changed their relative distances a little.

OD = oildrop.
pv = prostomal thickening
per = periblast.
Bl = blastoderm.
D = pavement layer
e = entoderm

Leiden, 17 January 1907.

[2]) Archives de Biologie T. 20. 1904. page 601.

J. BOEKE. On Gastrulation and the covering of the yolk in the teleostean egg.

PLATE 1.

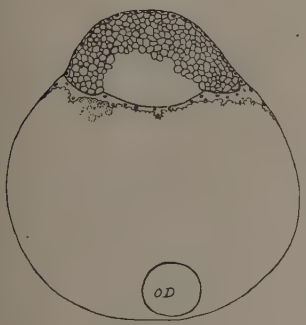

Fig. 1.

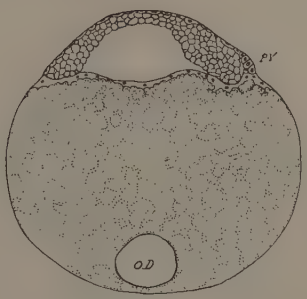

Fig. 2.

Fig. 3.

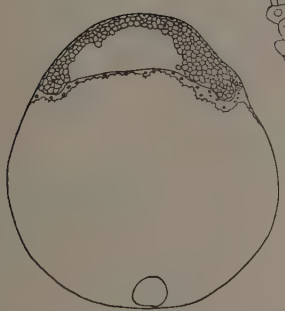

Fig. 4.

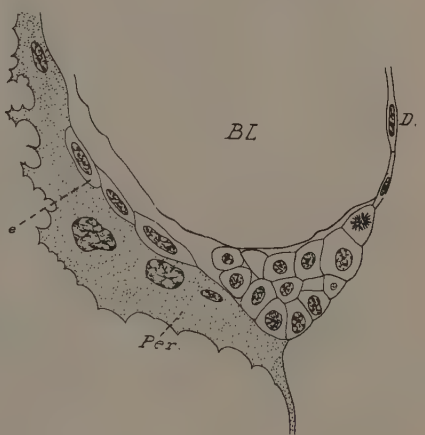

Fig. 5.

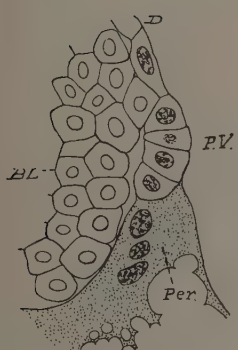

Fig. 6.

Fig. 7.

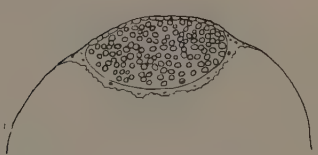

Fig. 8.

Fig. 9.

J. Boeke del.

PLATE 2.

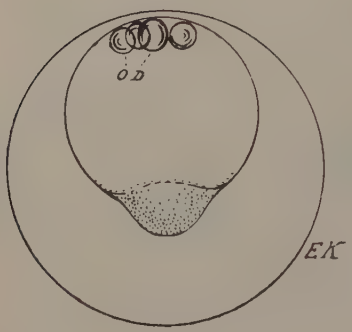

Fig. 1*a*.

Fig. 1*b*.

Fig. 1*c*.

Fig. 1*d*.

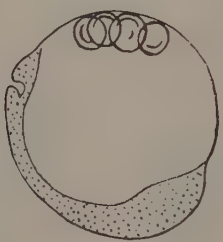

Fig. 1*e*.

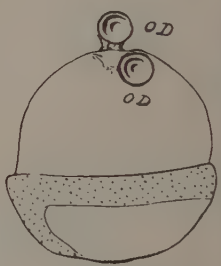

Fig. 2*a*.

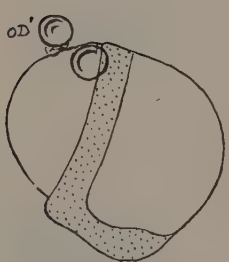

Fig. 2*b*.

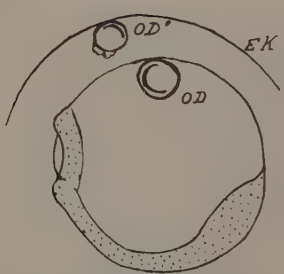

Fig. 2*c*.

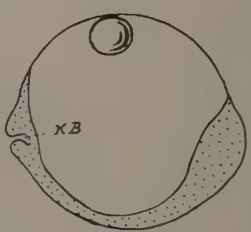

Fig. 2*d*.

Fig. 3.

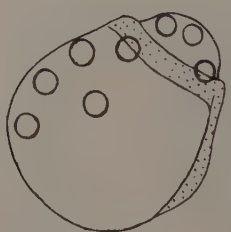

Fig. 4.

J. Boeke del.

Physics. — *"On the influence which irradiation exerts on the electrical conductivity of Antimonite from Japan"*. By Dr. F. M. JAEGER. (Communicated by Prof. P. ZEEMAN).

(Communicated in the meeting of February 23, 1907).

§ 1. Having been occupied for a considerable time with the determination of the specific electrical resistance in the three crystallographic main directions of the *antimonite* from Shikoku (Japan), I had already found that with this substance, which belongs to the very bad conductors, inexplicable irregularities presented themselves, when the resistance was determined several times anew during a long time, with identical electromotive force.

Generally the obtained deflection of the galvanometer first became larger and larger, and decreased again in course of time, after which, as I found, periodical increase and decrease sometimes followed. It was impossible to detect any connection between tension, intensity of current, and time.

As for rods of a length of some centimeters and a section of about a quarter of a square centimeter, resistances were found in the different directions lying between 500 and 20000 millions of Ohms, I first thought of an impregnation of the electrical charge in the ill-conducting material. On account of its opposed direction, however, an eventual polarisation current would have to cause an apparent *increase* of the resistance, whereas experience generally showed a *decrease* of the initial resistance.

§ 2. While I was trying to ascertain the cause of these deviations, a sunbeam fell through an aperture of the curtain on the piece of mirror-glass which closed the THOMSON-galvanometer, and was partially reflected to the apparatus containing the piece of antimonite, cut with its longitudinal direction parallel to the crystallographic *b*-axis. The needle of the galvanometer deflected immediately towards that side in which the total deflection was *increased*. At first I thought that the heat of the sun penetrating the galvanometer on one side had changed the cocoon thread so much as to cause a torsion. Some moments later, however, when I happened to light a match in the neighbourhood of the preparation, the *increase* of the already existing deflection was repeated, and now in the same sense as before, and at the same time stronger.

§ 3. So we have met here with a new phenomenon. Either the

radiation of light, or the heat must be the cause of the phenomenon.

I then undertook the following set of experiments.

A rod of antimonite quite covered by paraffin, and cut parallel to the *b*-axis, was shunted into the circuit of a dynamo, the tension being kept at exactly 35 Volts by means of a resistance of incandescent lamps. When shunting in the THOMSON-galvanometer [1]), which had been hung up in an antivibration apparatus of JULIUS, and which was so sensitive, that at a distance of mirror of two meters, it still gave a deflection (double) of 26,5 mm. for a current of 0,000000006 Ampères, — we obtained a constant, single deflection of 10,7 cm. on the left of the zero point.

An incandescent lamp (of 110 Volts), placed at about 2 meters' distance from the preparation, gave an *increase* of this deflection of 4 m.m., i. e. 3,7 % — agreeing in this case with a decrease of resistance of about 53 millions of Ohms.

When the same lamp was placed at 1 meter's distance it brought about an increase of the deflection of 11 m.m.; at $^1/_2$ meter's distance of about 20 m.m., and held near the rod for a short moment, of more than 220 m.m., i. e. an increase of the conductivity of resp. ± 10 %, ± 18,7 % and 206 %! [2])

Then the lamp was removed, and after the deflection had resumed about its original value, one of the curtains at the window was drawn aside, so that the diffuse daylight (overcast sky) fell on the apparatus. Instantly the deflection was increased by more than 4 m.m. i.e. about 3,7%. Then a wooden box was placed over the apparatus, and then removed. Every time the experiment was repeated the constant deflections in the light were found from 3 to 8 m.m. *larger* than those in the dark.

§ 4. In the foregoing experiments only exceedingly little light fell on the rod of antimonite, as it was quite covered by a coat of paraffin [3]) about 0,4 c.m. thick, and so only the light penetrating the half transparent coating could have any effect.

Then the experiment was repeated as follows.

A lamella [4]) of antimonite was clasped between two much larger copper plates, which two plates were well insulated. The condensator (fig. 1) obtained in this way was suspended on silk threads. [3])

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[1]) The internal resistance of this instrument amounted to 6681 Ohms.

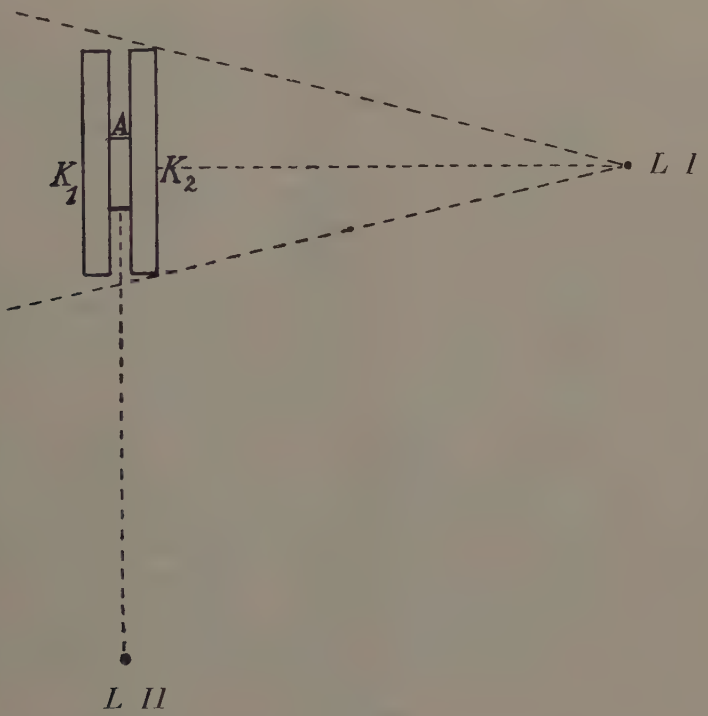
[2]) The resistance of the rod was diminished by an amount of more than 950 millions of Ohms in the latter case.

[3]) The purpose of these precautions will be explained later on in a paper written in conjunction with Mr. VAS NUNES.

[4]) The antimonite splits perfectly // (010), so ⊥ *b*-axis.

The antimonite plate had a thickness of about 1 m.m. and a section which may have amounted to about $^1/_4$ c.m^2.

Now if a source of light was placed at I, the remaining deflection of about 1,8 c.m. obtained at a 10,5 Volts' tension was only increased by 2 m.m., i. e. by about 11 %. If, however, the light was placed *at the same distance* in II, the increase amounted to about 11,5 m.m., i. e. 64 %.



In the former case the plate A is viz. in the shadow of K_2, and so receives but very little light reflected by the walls; in the latter case, however, the radiation is direct.

§ 5. If a thick plate of colourless plate-glass is placed between the source of light in II and the apparatus, the remarkable fact presents itself that the deflection is considerably *increased*. The explanation of this phenomenon was obvious. For a copper bar, heated to some hundreds of degrees, and brought near the apparatus, immediately *diminished* the obtained deflection greatly. Hence — and this is a most remarkable result — *rise of temperature has an influence directly opposed* to that of radiation of light: it enlarges the resistance instead of diminishing it, as rays of light do.

If the plate is again removed, the deflection decreases again to the value it had before the plate was placed between etc. So this fully proves that it is the *radiation of light* which influences the conductivity of antimonite in so high a degree, and *not* the heat; for the latter diminishes the conductivity, in contrast with the former.

§ 6. Finally glass plates of different colours were interposed between the source of light and the rod.

It then appeared that the influence of the differently coloured light was very different. The antimonite namely proved to be subjected to hardly any change by *green* light; for *red* light the increase was pretty large, for *yellow* light a little more, for *green* very small, for *violet* light again stronger. In each of these cases the deflection appeared to have resumed its original value after removal of the source of light[1]). With violet radiation I obtained an increase ot conductivity which amounted to about 150 % of the original value; with white light with interposed glass plate one of about 250 %.

To get some insight into the *quantitative* action for a special case, the following experiments were made. An ordinary electrical incandescent lamp was adjusted at 16 cms'. distance from an antimonite rod covered with a coat of paraffin 1 cm. thick. First of all it was ascertained that action of light by itself did not excite an electrical current. It then appeared that the deflection of the galvanometer was increased just as much irrespective of the direction of the current. So the decrease of resistance is independent of the direction of the current. By interposition of coloured glass plates I got a rough estimation of the relative influence of the different colours of the spectrum. Thus I found:

White	light,	placed	at	16cms'.	distance,	makes	the	conductivity	rise	to	200%	of	its	original	value				
Red	"	"	"	"	"	"	"	"	"	"	"	194%	"	"	"	"			
Orange	"	"	"	"	"	"	"	"	"	"	"	153%	"	"	"	"			
Green	"	"	"	"	"	"	"	"	"	"	"	116%	"	"	"	"			
Blue	"	"	"	"	"	"	"	"	"	"	"	176%	"	"	"	"			

[1]) Not quite the original value. The substance shows hysteresis to a certain amount, which, however, is smaller than for selenium. Already 20 à 40 minutes after the source of light had been removed, the original deflection was sometimes found back. The mineral seems to be quite free from any admixture of selenium, as a qualitative investigation taught me. Remarkable in a high degree is the fact, that on melting the natural mineral, it obtains, when solidified as a conglomerate of little crystals, a specific resistance, which is many thousand times less than before, while at the same time it has lost its sensibility to light-radiation quite. On heating the antimonite however, without melting, it conserves this property. Analysis has taught me, that there are present the elements: Sb, S, Ca, Ba, Sr, Si and, as Prof. KLEY found, traces of Zn and Co; also SiO_2-crystals are included. (Added in the English translation).

As heat-rays have only an exceedingly slight effect, and, as I ascertained later on in conjunction with Mr. VAS NUNES, also the ultraviolet light emitted by cadmium poles causes only a small increase of the conductivity, the dependence of wave-length and decrease of resistance is evidently represented by a curve whose minima lie in the ultrared, in the green and in the ultraviolet, and whose maxima are situated in the red and in the blue part of the spectrum [1]).

Later on when the determinations of the resistance of this substance will have led to favourable results, we shall make some closer communication on the relation between thermal and electric motion in this conductor.

§ 7. The phenomenon discovered here reminds strongly of that observed for *selenium* [2]). It is, however, noteworthy, that though the dependence of the increase of the conductivity on the radiation of light, and even on the wave-length of the light manifests itself in a perfectly analogous way to that for antimonite, yet the two differ in some respects. First of all for the selenium *polymorphous changes*, and the displacement of equilibrium attending them play an important part; then, however, the resistance always decreases here with rise of temperature, so exactly the reverse of what happens in my investigations, in which moreover there is no question whatever of polymorphous changes, as far as is known. An analogy between the two cases is to be found in the fact already discovered by ADAMS [3]), that the resistance decreases with rising electromotive force, also after continued action of it; such a deviation from the law of OHM is also found for the antimonite.

On the other hand the behaviour of antimonite from Japan seems to present a closer analogy with that of the crystallised *tellurium;*

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[1]) Though it is not intended as an explanation, I will yet call to mind that it follows from MÜLLER's investigation (N. Jahrb. f. Miner. u. s. w. Beil. Bd. **17**, 187—251) on the optical constants of the antimonite from Japan, that the indices of refraction n_1 and n_2 have their maximum values exactly for the *green* rays (between the lines E and F) (viz. $n_1 = 5{,}47-5{,}53$ and $n_2 = 4{,}52-4{,}49$), while also the double refraction reaches its maximum value for these rays. The polarisation of the reflected rays is right-elliptic (negative). However, on using polarized light, we could not find any influence of the direction of vibration: the variation of the electric resistance was in the two cases the same. (Added in the English translation).

[2]) G. WIEDEMANN, Die Lehre v. d. Elektricität. (1882). **I**. p. 544—553).

[3]) SALE, Phil. Mag. [4]. **47**. 216. (1874); Pogg. Ann. **150**. 333; Chem. News. **33**. 1. (1876).

[4]) ADAMS, Phil. Trans. **157**.; Pogg. Ann. **159**. 621. (1876), Phil. Mag. [5]. **1**. 115

here, too, the resistance increases with heating, decreases with exposure to light [1]).

In conjunction with Mr. VAS NUNES I hope shortly to publish also some quantitative data on the phenomenon discovered by me, and also on the behaviour of the melted and again solidified antimonite and the analogous selenium compound. This investigation has been made in the Physical Laboratory at Amsterdam.

Anatomy. — *"On the influence of the fins upon the form of the trunk-myotome"*. By B. VAN TRICHT. (Communicated by Prof. G. C. J. VOSMAER). (From the Anatomical Institute at Leyden).

(Communicated in the meeting of March 30, 1907).

This research forms a direct sequel to Professor LANGELAAN's work "On the Form of the Trunk-myotome", and is intended to show the influence of the fins upon the form of the myotome. The method which I followed, was based upon the chief result of the foregoing research viz. that the differentiation of the myotome takes place in a continuous manner by means of folding, and that it is possible to follow the process of folding in dissecting the intermyotomal tissue. Now the method of direct dissection proved to be restricted in its application, so that it was necessary partly to apply a more indirect one. This latter method rests upon the relation, which exists between the form of the myotome and the structure of the transverse sections of the animal.

Differentiation of the dorsal musculature.

From a rather large sample of Mustelus vulgaris the skin with the underlying connective tissue was removed, so that the external surface of the myotoms was laid bare (figure I). Then in the region before the first dorsal fin the parts constituting one and the same myotome were determined; the form of this myotome exhibited about the same form as in Acanthias, only the lateral part of the myotome proved to be displaced caudally; the breadth of this displacement amounted to about half the breadth of the myotome. This myotome was arbitrarily indicated by the number

[1]) I have made an arrangement with Mr. J. W. GILTAY at Delft with regard to the mounting of antimonite preparations, and the preparation of antimonite cells for practical use.

1 and the following myotoms by subsequent numbers. After that, transverse sections of the animal were made, of 1—2 cm. thickness, and the numbering transferred to these sections, so that the lamellae belonging to one and the same myotome received the same number. For the sake of an easy description, figure II gives a hemischematic representation of the myotome, in which the peaks are indicated by numbers, the lamellae by letters. In figure III which is the transverse section, (indicated in figure I with an *A*) the peaks appear as systems of concentric lamellae marked in accordance with the marking in figure II. If we now pass to the region of the first dorsal fin (figure IV, section *B* of figure I and fig. V, section *C* of fig. I) the image of the transverse section is changed, instead of being composed by four peaks, the dorsal musculature only shows three peaks.

The peak, indicated as number 1, has disappeared and instead of this peak we find the first lamellae of the dorsal fin. Now in all subsequent sections this first peak does not reappear any more. By the method of successsive numbering it was possible to determine the first myotome losing its most dorsal peak (number 1). The external surface of this myotome is blackened in figure I. From the principle laid down in the beginning of this notice ensues, that the myotome apparently losing its first peak, gives a muscular element to the dorsal fin; this element is therefore also blackened in figure I. It may be seen in figure I that the first myotome giving an element to the fin lies a little caudally in respect to the front edge of this fin. The number of myotomes giving a part to the first dorsal fin may easily be determined, because these composing parts of the fin are separated by intersegmental tissue; that we have really to deal with intersegmental tissue follows from the fact that through these lamellae bloodvessels and extremely fine nerve fibres reach the skin (vid. v. Bisselick "On the Innervation of the Trunk-myotome"). The total number of muscular elements composing the fin, amounted to 34, so that the last myotome still giving an element to the first dorsal fin already lies in the region of the second dorsal fin. From the fact that the most dorsal peak (number 1) does not reappear any more in the transverse sections, it follows that the next myotome gives the first element to the second dorsal fin. The surface of this myotome is also blackened in figure I to show its position in relation to the front edge of the second dorsal fin. It is evident, that this myotome occupies the same position in respect to the second dorsal fin as the first myotome does in respect to the first dorsal fin. The number of myotomes composing the second dorsal fin amounts to 30.

Upon the second dorsal fin follows the dorsal part of the caudal

fin. In this fin the myotomes are pressed together so closely that a direct counting of the number composing the fin is no more possible ; by comparing the total number of vertebrae to the number of myotomes composing the first and second dorsal fins, we find that about 70 myotomes give an element to the dorsal part of the caudal fin.

The results obtained by this indirect method are corroborated by the result of the direct dissection. If we take a myotome giving a muscular element upon the more anterior part of the dorsal fin and begin the dissection with lamella *b* in the neighbourhood of the second peak and proceed preparing caudally, we find lamella *b* being rolled in, towards the mesial plane of the body, in the shallow excavation in which the base of the fin rests, (fig. VI). Along this way the muscular tissue becomes gradually atrophic and only a thin band remains, consisting of the connective tissue which forms the framework for the muscle fibres. In the neighbourhood of the sagittal plane of the body this lamella is folded, in such a manner, that the line of folding (figure VI L″ L″) runs parallel to the sagittal axis of the body. By this process of infolding the direction of the lamella *b* is reversed, the infolded part proceeding cranially; this part of lamella *b* passes into the dense sheath of connective tissue, which is interposed between the dorsal musculature and the base of the fin. As far as I can see this sheath of connective tissue is chiefly built up by a large number of these lamellae, but they are so inextricably united that I have not been able to follow lamella *b* in this sheath. If starting from the fin, we prepare free one muscular element of the fin, and this element is lifted up with enough precaution, it may be seen, that from the base of such a fin-element as well a thin lamella of connective tissue passes into that sheath of tissue in which we could follow the reversed part of lamella *b*. The direct continuity however of both lamellae in the sheath of dense connective tissue, I have not been able to establish.

The muscular elements composing the fin (figure VI) are triangular laminae; one side of the triangle is contiguous to the fin-rays and the connective tissue which unites these rays in the mesial plane of the body, the lateral side forms part of the lateral surface of the fin, while the base is excavated and moulded upon the shallow depression in the dorsal musculature. From the outside a septum of intermyotomal tissue (s. i. figure VI) penetrates into the muscular substance of the fin dividing this substance into a lateral (*b*) and a mesial part (*a*). This septum inserts a little above the muscular substance upon the fin-rays, and becomes thinner and thinner without

reaching the base of the fin. At the base therefore the lateral and mesial parts of the muscular substance are continuous and form a peak (figure VI p. 1), lying quite near the mesial septum of the body. This peak must therefore represent the peak which is lost in the transverse section (figure IV) made at the level of the first dorsal fin. The septum penetrating into the muscular substance of the fin is therefore the intermyotomal septum stretched out between lamellae *a* and *b* of figure II.

It ensues therefore from the combined observations, that the first dorsal fin (and the same applies to the other dorsal fins) is differentiated by a process of infolding similar to the differentiation between the dorsal and the lateral and between the lateral and the ventral musculature. The line of infolding crosses lamella *b*. In that part of the lamella, which lies in the depth of the fold the muscular tissue is atrophic. Proceeding from peak 2 caudally along lamella *b* the atrophy of the musclefibres gradually increases, whilst on the other hand proceeding from peak 1 caudally along lamella *b* (as far as it lies upon the fin) the atrophy of the muscular tissue is abrupt. The position of peak 1 has not changed in respect to the mesial plane of the body, only the lamellae have changed their direction. The superior cornu (i. e. lamella *a*) is no longer directed cranially but turned upwards and this is also the case with that part of lamella *b* that has passed into the fin. In connection with this representation of the facts, I determined the direction of the muscle fibres in the fin; here they slope downwards from the intermyotomal septum. Now if we imagine the lamellae composing the fin restored to their original position, the course of the fibres in lamella *a* would be from mesial to lateral and from caudal to cranial and this was actually the direction of the muscle fibres in lamella *a* in the region cranially of the first dorsal fin.

Differentiation of the latero-ventral musculature.

The lateral musculature, as described by VAN BISSELICK, shows a peak directed caudally (peak 5, figure II and fig. VII) situated near the second line of infolding L'L'. Proceeding along the body a second peak appears directed cranially. The first myotome showing this peak (peak 6, fig. II and fig. VII), is the eleventh myotome following the first myotome giving a muscular element to the first dorsal fin. The two peaks lie near to each other in the neighbourhood of the second fold. In consequence of the infolding of the myotome at that place, they do not reach the surface of the body, being covered from the outside by the ventral musculature. Meanwhile the ventral part of the myotome undergoes a change in form, the first lamella belonging

to the ventral musculature (lam. *f* figure II and fig. VII) becomes shorter and the first peak of the ventral musculature directed cranially (peak 7, figure II), more and more develops into a true peak. Now by the disappearence of lamella *f* peak 6 and 7 approach each other, remaining divided, however, by a thin lamella of connective tissue penetrating into the second fold along the line L'L' (fig. VIII and IX). In consequence of the process of infolding peak 6 lies mesially in respect to peak 7 which covers peak 6 from the outside. At the level of myotome 15 (reckoned from the first myotome, giving an muscular element to the first dorsal fin) the second fold vanishes. Together with the disappearence of the fold we notice the vanishing of the displacement of the lateral musculature in respect to the ventral musculature, which was only a consequence of the process of infolding, so that the two peaks (6 and 7) lie side by side in the same transversal level of the body. At the place of disappearance of the second fold the two peaks unite to a single peak directed cranially. Together with the disappearance of lamella *f* we notice the further development of lamella *g*.

At the same level where the second lateral fold disappears, we find the appearance of the cartilagineous plate, uniting the two basipterygii of the pelvic fins. With its front border, this plate folds in lamella *g* (figure II and X) from the inside so that this lamella covers the front edge of this plate; in this way the pelvic fin is formed. The details of the formation of the pelvic fins I have not yet investigated. By the formation of the pelvic fin peak 8 (fig. II) and lamellae *g* and *h* pass into the musculature of the fin, so that in a transverse section through the animal, at the level of the pelvic fin, the trunkmusculature is only composed by five peaks (viz. 2. 3. 4. 5. (6 + 7) of figure II). This structure of the transverse sections does not change any more proceeding along the body caudally (figure XI and XII).

The disappearance of the first fold, dividing the dorsal from the lateral musculature, takes place in the same way as described for the second fold, at the level of myotome 45 (reckoned from the first myotome giving an element to the first dorsal fin); only the case is more simple not being complicated by the presence of two peaks.

Finally I paid attention to the influence of the abdominal cavity upon the form of the myotome. I found this influence to be very restricted, as it only determines to some extent the dimensions of the myotome, without producing any particular differentiation in its form.

In fine I wish to express my thank to Prof. LANGELAAN for his aid and assistance in these researches.

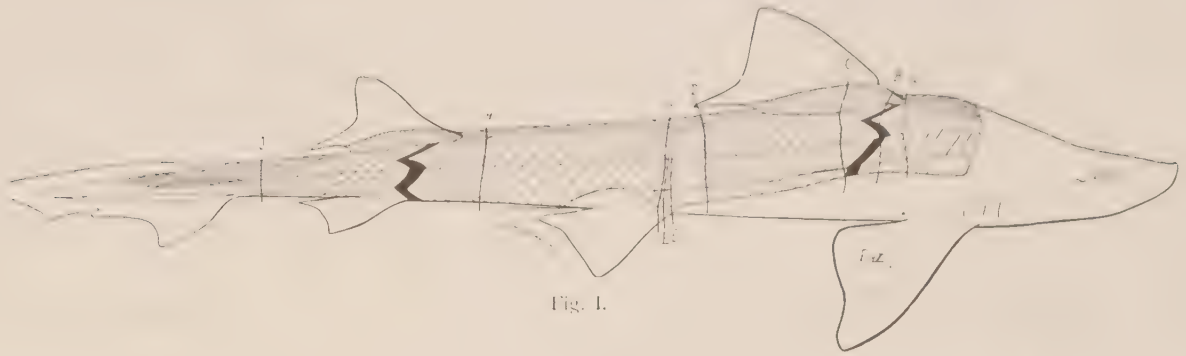


Fig. I.

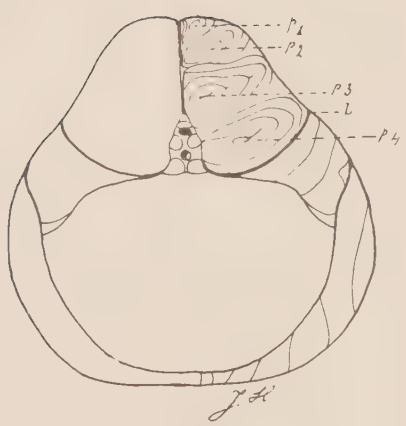


Fig. III.

Fig. IV.

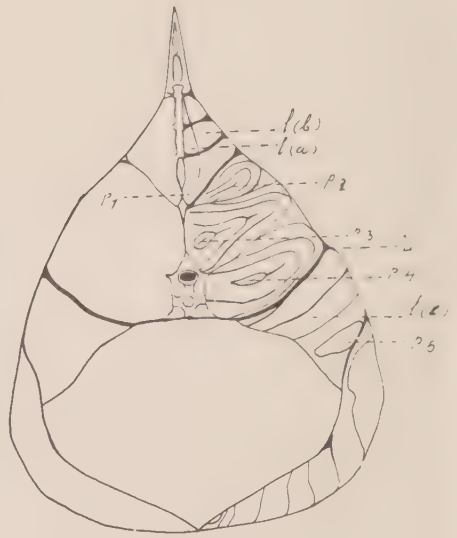
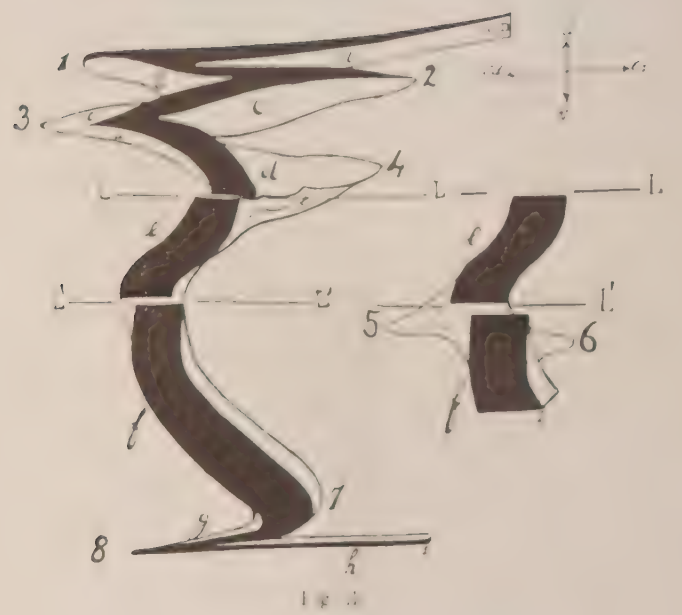
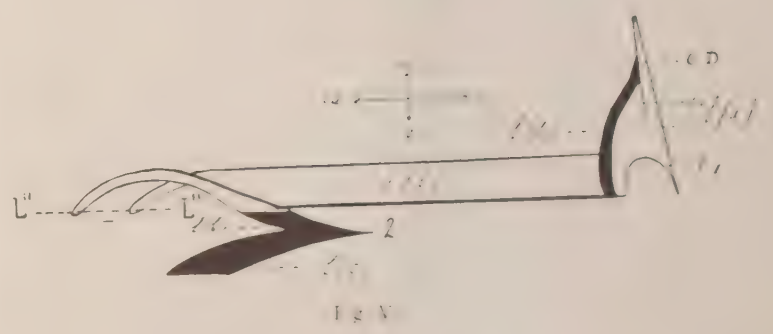


Fig. V.





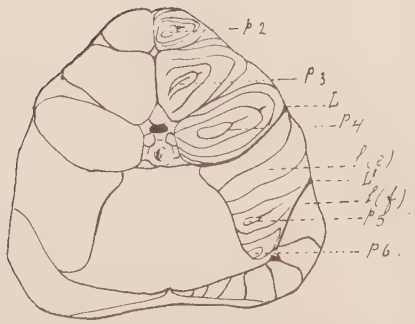


Fig. VII.

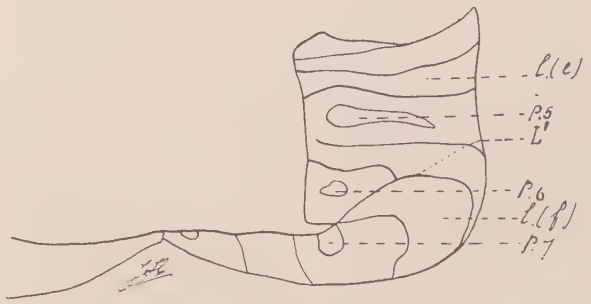


Fig. VIII.

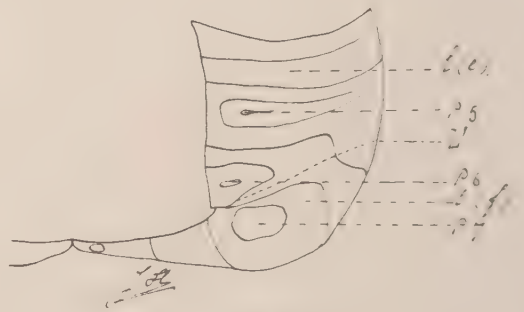


Fig. IX.

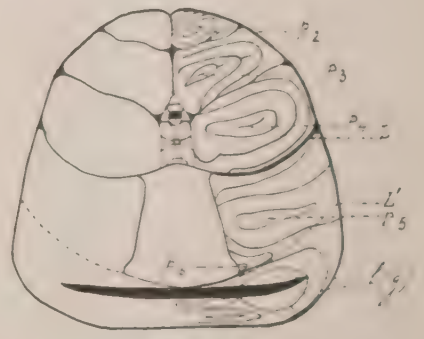


Fig. X.

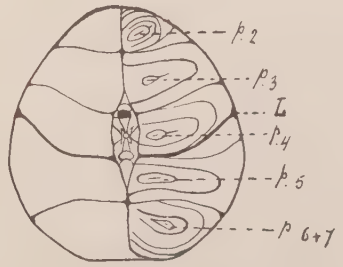


Fig. XI.

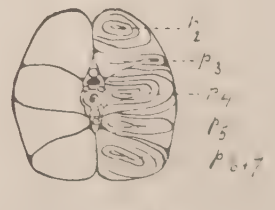


Fig. XII.

Anatomy. — *"Anatomical Research about cerebellar connections."* (Third communication). By Dr. L. J. J. MUSKENS (Communicated by Prof. C. WINKLER).

(Communicated in the meeting of March 30, 1907).

The ventral cerebello-thalamic bundle.

Whereas it is nowadays generally accepted, that the direction of conduction in the superior crus cerebelli is cerebellofugal, there is no uniformity of opinion attained yet by the authors regarding the bundle, which is found degenerated in the predorsal region in the pons after cerebellar lesions. After PELLIZZI and VAN GEHUCHTEN, THOMAS, ORESTANO, CAYAL and LEWANDOWSKY this bundle is built up by fibres, which take their origin from the superior crus after it has crossed the raphe in WERNEKINCK's commissure, the direction of conduction being rubro-fugal. PROBST however, describes this bundle as the ventral cerebello-thalamic bundle, conducting nervous impulses from the cerebellar basal nuclei upward towards the red nucleus.

The problem of this bundle really has to deal with two questions; 1st. which are the two nerve centres, which are connected by means of this bundle and 2nd. what is the direction of conduction of impulses in the same.

Cats appear to be more suitable for these experiments. In two animals different parts of the cerebellar cortex, with the adjacent part of the basal nuclei, were removed, except the flocculus. In these animals there was hardly any degeneration at all in the ventral cerebello-thalamic bundle, whereas in 3 other cats in which with other parts also the flocculus was removed there was very extensive degeneration of this bundle. That these fibres do not take their origin from the cortical gray matter of the flocculus is proved by the fact, that in another cat in which the cortex of the formatio vermicularis cerebelli was injured, no degeneration of the said bundle was found.

In two cats (XXIII and LXI) a lesion was effected in the midbrain, by passing a curved knife in front of the lobus simplex cerebelli in such a way, that the predorsal region on the right side was cut, distally from the red nucleus. In none of these animals any degeneration was found in the ventral bundle. If CAYAL's supposition were correct, certainly a great many of the descending collaterals of the superior crus ought to have been found degenerated.

In one cat (LVIII) a longitudinal lesion was effected in the teg-

mentum, the instrument (PROBST's covered hook) passing through the middle crus cerebelli. In this cat were found a certain number of fibres degenerated, which passed through the regio reticularis of the side of the lesion and then, crossing the raphe and running upward in the predorsal region of the other side, took their way towards the red nucleus. This experiment tends to show, that there are direct fibres, coming from the basal cerebellar nuclei, which do not join the superior crus, but follow the ventral course to arrive at the red nucleus. LEWANDOWSKY,S fibres O. P. (in fig. 66 and 37) are not to be identified with these fibres on account of their entirely different course.

In cat LXII the anterior crus cerebelli was partially cut, and at the same time an incision made into the middle crus. Also in this animal there was found no degeneration on the distal side of the lesion except the bundle of Monakow. Were the ventral bundle to be regarded as being formed by descending collaterals of the anterior crus this result could hardly be explained. A simular result was obtained in cat LXVIII, where hemisection of the pons was effected. Also here there was no degeneration on the distal side of the lesion.

Mathematics. — "*Equilibrium of systems of forces and rotations in* Sp_4." By Dr. S. L. VAN OSS. (Communicated by Prof. P. H. SCHOUTE).

(Communicated in the meeting of March 30, 1907).

Referring to the following well-known properties:

a. The coordinates p_{ij} and π_{ij} of a line p and a plane π satisfy the five relations:

$$P_i \equiv p_{kl}\,p_{jm} + p_{lj}\,p_{km} + p_{jk}\,p_{lm} = 0 \quad , \quad \pi_i = \sum_i \pi_{kl}\,\pi_{jm} = 0, \quad . \quad (1)$$

of which relations three are mutually independent.

b. The condition that a line p and a plane π intersect each other is expressed by

$$\Sigma\, p_{ij}\,\pi_{ij} = 0\,. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

c. The coordinates of the point of intersection X of two planes π, π' and that of $Sp_2\Xi$ through two lines p, p' are:

$$x_i = \pi_{kl}\,\pi'_{jm} + \pi_{lj}\,\pi'_{km} + \pi_{jk}\,\pi'_{lm} + \pi_{jm}\,\pi'_{kl} + \pi_{km}\,\pi'_{lj} + \pi_{lm}\,\pi'_{jk};$$

$$\xi_i = \sum_i p_{kl}\,p'_{jm}\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

we wish to draw the attention to the following properties:

If (ij) are ten *arbitrary* quantities: i.e. not satisfying the relations

$\sum_{\iota} (jk)(lm) = 0$, we shall continually be able to break up each of these quantities into two parts $(ij)'$ and $(ij)''$, so that $\sum_{i} (kl)'(jm)' = \sum_{i} (kl)''(jm)'' = 0$. It is easy to see that this decomposition can be done in ∞^4 ways.

For each decomposition holds good:

$$\sum (kl)'(jm)'' = \sum (kl)(jm) \quad , \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

for:

$$\sum_{\iota} (kl)'(jm)'' = \sum_{i} (kl)'\{(jm) - (jm)'\} = \sum_{i} (kl)'(jm),$$

likewise:

$$\sum_{i} (kl)'(jm)'' = \sum_{i} (kl)''(jm),$$

from which by addition:

$$\sum_{i} (kl)'(jm)'' = \sum_{i} (kl)(jm).$$

Giving a geometrical interpretation we regard a homogeneous system of 10 arbitrary quantities a_{ij} and α_{ij} as the coordinates of

a system a of ∞^4 lines, in pairs a', a'' conjugated by the relations:

$$a'_{ij} + a''_{ij} = a_{ij} \quad . \quad . \quad (5)$$

All these lines lie in one Sp_5 Ξ having as coordinates:

$$\xi_i = \sum_{\iota} a_{kl}\, a_{jm} \quad . \quad . \quad . \quad . \quad (6)$$

a system α of ∞^4 planes, in pairs α', α'' conjugated by the relations:

$$\alpha'_{ij} + \alpha''_{ij} = \alpha_{ij} \quad . \quad . \quad . \quad . \quad (5')$$

All these planes pass through one point X, having as coordinates:

$$x_\iota = \sum_{\iota} \alpha_{kl}\, \alpha_{jm} \quad . \quad . \quad . \quad . \quad (6')$$

We now annul the homogeneousness of the p-, π-, a- and α-coordinates.

This causes those elements to assume vector-nature and makes them interpretable respectively as force, as rotation, as dynam and as double-rotation. The equations (5), (5′) determine the reduction of the *vectors* a and α on the conjugate pairs of lines and of planes of the systems a and α under consideration and not yet partaking vector-nature, whose structure now becomes revealed.

II. In connection with the meaning given in b of the equation $\sum p_{ij}\, \pi_{ij} = 0$ we interpret

$$\sum a_{ij}\, p_{ij} = 0 \quad . \quad . \quad . \quad . \quad (7)$$

as the condition that a line p cuts a pair of conjugated planes of system α.

$$\sum a_{ij}\, \pi_{ij} = 0 \quad . \quad . \quad . \quad . \quad (7')$$

as the condition that a plane π cuts a pair of conjugated lines of system a.

This gives us a very fair survey of the structure of the linear complex of lines and planes. The reduction of the equation of the complex of planes to its diametral space is now easy to do; likewise

the further reduction to the simplest form $(kl) = \sigma(jm)$, assumed by the equation when the edges kl and jm, the planes ijm and ikl of the simplex of coordinates are conjugated elements of the systems a or α.

III. If we assign to the elements p, π, a, α vector-nature, expressions $\Sigma a_{ij} p_{ij}$, $\Sigma \alpha_{ij} \pi_{ij}$ become of importance as *virtual coefficients* (in BALL's theory of screws) and the disappearing of these coefficients then gives the *condition that the force p performs no work at a displacement in consequence of a double rotation a, resp. that the dynam α performs no work at a rotation π.*

So in BALL's notation the equations (7), (7)′ give the *condition of reciprocity between force and double rotation, resp. between dynam and rotation.*

In like manner the equation

$$\Sigma a \; \alpha_{ij} = 0 , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which includes (7) and (7)′ and likewise (2), gives the condition of reciprocity between the dynam a and the double rotation α.

IV. We shall now pass to the general equilibrium of forces and rotations. It will be convenient to understand by p, π, α, a vectors *unity* and to indicate the intensity of these vectors by a factor.

It will be sufficient to limit ourselves to the equilibrium of forces, leaving the treatment of the dual case to the reader.

In the first place we regard the case of n forces, $n > 10$ working along lines given arbitrarily.

It goes without saying that for the equilibrium it is necessary and sufficient that the intensities $k^{(\nu)}$ satisfy the ten conditions:

$$\Sigma k^{(\nu)} p^{(\nu)} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

We can therefore in general bring arbitrary intensities along $n - 10$ vectors, those on the other ten then being determined by the above equation (9).

In particular for $n = 11$ the theorem holds:

To vectors along eleven lines given arbitrarily belongs in general only one distribution of ratios of intensity, so that the system on those lines is in equilibrium.

The generality of the case is circumscribed by the requirement that no ten lines can satisfy one and the same linear condition in the form $\Sigma a_{ij} p^{\nu}_{ij} = 0$, where the coefficients a_{ij} do not depend on ν, in consequence of a well-known property of determinants tending to zero.

So if there are among n lines at most 10 belonging to a linear complex we can satisfy the equations (9) by choosing all intensities except those belonging to these 10 equal to 0 and then (if not all subdeterminants of order 9 tend to 0) we shall be able to bring along these last only *one* distribution of intensity differing from 0 in such a way that the system of forces obtained in this manner is in equilibrium.

We have thus at the same time arrived at the following theorems:

For the equilibrium of ten forces it is necessary that these belong to one and at most to one linear complex. In this case always one and not more than one distribution of intensity is possible.

If we continue the investigation of the equations (9) we then obtain successively the conditions of equilibrium of 9, 8, 7, 6, 5 forces. We can express the result as follows:

In order to let n forces, $11 > n > 4$, *admit only of one distribution of intensity in equilibrium, it is necessary and sufficient for them to be the common elements of exactly* $21-n$ *linear complexes.*

In particular for $n = 5$ we find the condition that the forces must belong to a system of associated lines of SEGRE.

This has given us a connection with a former paper in which we treated this case synthetically.

V. The condition that ten forces in equilibrium belong to one complex follows almost immediately out of the interpretation of the equation $\Sigma a_{ij} p_{ij} = 0$ as condition of reciprocity of force and double rotation.

Let e.g. ten forces be given in equilibrium; nine of these forces chosen arbitrarily determine a complex, so also the double rotation α for which none of them can perform labour. The united system of ten forces, as being in equilibrium doing no labour for no motion whatever, it is necessary for the tenth force to be likewise reciprocal with respect to the double rotation α, i.e. this force belongs with the former nine to the selfsame complex.

Equally simple is the deduction of the conditions of equilibrium for nine forces.

For eight forces determine a simply-infinite pencil of complexes whose conjugate double rotations $\alpha + \lambda\alpha'$ are all reciprocal with respect to these eight forces. So they must also be reciprocal with respect to the ninth force in equilibrium with these, i. o. w. the latter must belong to all linear complexes to which the eight others belong.

And so on.

VI. We shall now denote still, by means of a few words, in which way we can arrive at an extension of the screw-theory of BALL by the application of the principle of exchange of space-element to the equations $\sum_{1}^{10} a_i \xi_i = 0$.

By interpreting this equation either

1st. as condition of united position of a point X and an Sp_3 Ξ in Sp_9,

2nd. as condition of reciprocity (BALL) of a dynam X and a double rotation Ξ,

we make a connection between the point- and Sp_3-geometry in Sp_9 on one hand and the geometry of dynams and double rotations on the other hand.

To each theorem of the former corresponds a theorem of the latter geometry. Nov the remarkable fact makes its appearance that *the fundamental theorems of the geometry of* Sp_6 *correspond to the fundamental theorems of the theory of screws of* BALL *in* Sp_3.

With this as basis we shall show, though it be but by means of some few examples of a fundamental nature, that the principles of a generalisation of the theory of screws are very easy to be arrived at by transcription of the simplest properties of the point- and Sp_3-geometry in Sp_9 which examples can at the same time be of service to explain the above observations on the theory of BALL in Sp_3.

To avoid prolixity we introduce the following notation. We call:

dynamoid the system of lines whose conjugate pairs can serve as bearers of a dynam.

rotoid the system of planes whose conjugate pairs can serve as bearers of a double rotation: So dynamoid and rotoid correspond to dynam and double rotation as in the notation of BALL "screw" to dynam and helicoidal movement.

Let the following transcriptions be sufficient to explain the application of the above principle.

σX:	Point X bearing a mass σ.	Dynamoid X bearing a dynam of intensity X.
$\sigma\Xi$:	Sp_3 Ξ with a density of mass σ.	Rotoid Ξ bearing a double rotation of intensity σ.
$(X'X'')$:	Right line, locus of the centres of gravity of variable masses in the points X' and X''.	Pencil of dynamoids, locus of the bearers of the resultants of two variable dynams on the dynamoids X' and X''.
$(\Xi'\Xi'')$:	Sp_3-pencil. A right line has always	Pencil of Rotoids. A pencil of dynamoids always

a point in common with an Sp_8.	contains a dynamoid reciprocal to a given rotoid.
An Sp_8 is determined by nine points.	A rotoid can always be determined lying reciprocal with respect to nine dynamoids.
p spaces Sp_8 cut each other according to Sp_{9-p}.	The dynamoids reciprocal to the movements of a body with p degrees of freedom form a $(9-p)$-fold infinite pencil.

Etc. etc.

We shall now apply the above to the problem: "To decompose a dynam according to *ten* given dynamoids", this problem being a transcription of the following:

"To apply to ten given points a distribution of mass so that the centre of gravity finds its place in a given point."

We again put side by side the results.

To be defined successively:

a. An Sp_8 through nine of the given points.	The rotoid reciprocal to nine of the given dynamoids.
b. The right line through the remaining point and the centre of gravity.	The pencil of dynamoids through the remaining dynamoid and the bearer of the given dynam.
c. The point of intersection of this right line with the Sp_8 found in *a*.	The dynamoid on this pencil reciprocal with respect to the rotoid found in *a*.
d. The decomposition of the mass in the centre of gravity according to this point of intersection and the 10th point named in *b*, which is possible, these three points being collinear; gives at once the mass to be applied in the last named point.	The decomposition of the given dynam according to the dynamoid found in *c* and the 10th dynamoid named in *b*, which is possible, these three dynamoids belonging to one pencil, gives at once the intensity of the dynam on the last mentioned dynamoid.
The other must necessarily become the centre of gravity of the remaining nine points.	The other must necessarily bear the resultant of the dynam to be applied to the remaining nine dynamoids.
e. These treatments to be repeated for the determination of mass in the other points.	These treatments to be repeated for the determination of intensity on the other dynamoids.

Zalt-Bommel, March 28, 1907.

Physics. — *"Contribution to the theory of binary mixtures," III,* by Prof. J. D. VAN DER WAALS.

Continued, see page 727.

We shall now proceed to describe the course of the spinodal curve and the place of the plaitpoints when choosing regions of fig. 1 which lie more to the right. But it has appeared from what precedes that to decide what different cases may occur, we must know the relative position of the curves $\frac{d^2\psi}{dx^2} = 0$ and $\frac{d^2\psi}{dv^2} = 0$, to which now the curve $\frac{d^2\psi}{dxdv} = 0$ is added; so the relative position at different temperatures of the three curves which occur in the equation of the spinodal curve.

The curves $\frac{d^2\psi}{dv^2} = 0$ and $\frac{d^2\psi}{dxdv} = 0$ may be considered as sufficiently known, and the knowledge of the relative position of these curves with regard to each other enabled us already before to elucidate sufficiently the critical phenomena of mixtures with minimum critical temperature — and though with regard to the relative position of these lines some particularities are met with, which have not expressly been set forth, I shall assume the properties of these lines to be known. But the curve $\frac{d^2\psi}{dx^2} = 0$ is less known — and it has appeared from the foregoing remarks, that if we wish to understand the occurrence of complex plaits, the relative position of this curve with respect to the curve $\frac{d^2\psi}{dv^2} = 0$ must be known. If this line lies altogether within the region where $\frac{d^2\psi}{dv^2}$ is negative, it has no influence to speak of on the course of the spinodal line, but if it lies either partially or entirely outside this region, the influence on the course of the spinodal curve is great, and the existence of this curve accounts for the complexity of the plait and gives rise to the phenomena of non-miscibility. I have, therefore, thought it advisable to investigate the properties of the curve $\frac{d^2\psi}{dx^2} = 0$, before proceeding to the description of the course of the spinodal curve also in other regions of fig. 1. A perfectly exact investigation of this line would, of course, require a perfectly accurate knowledge of the equation of state. But

the value assumed already before as an approximate equation of this line:

$$\frac{d^2\psi}{dx^2} = MRT\left\{\frac{1}{x(1-x)} + \frac{\left(\frac{db}{dx}\right)^2}{(v-b)^2}\right\} - \frac{\frac{d^2a}{dx^2}}{v} = 0$$

will prove adapted to give an insight into the different possible positions of this line with respect to $\frac{d^2\psi}{dv^2} = 0$ and $\frac{d^2\psi}{dxdv} = 0$.

THE CURVE $\frac{d^2\psi}{dx^2} = 0$.

The differential equation of this curve:

$$\frac{d^3\psi}{dx^3}dx + \frac{d^3\psi}{dx^2dv}dv + \frac{d^3\psi}{dx^2dT}dT = 0$$

may also be written in the following forms:

$$\frac{d^3\psi}{dx^3}dx + \frac{d^3\psi}{dx^2dv}dv - \frac{d^2T\eta}{dx^2{}_vT}\frac{dT}{T} = 0$$

or

$$\frac{d^3\psi}{dx^3}dx + \frac{d^3\psi}{dx^2dv}dv - \frac{d^2(\varepsilon-\psi)}{dx^2{}_vT}\frac{dT}{T} = 0$$

or

$$\frac{d^3\psi}{dx^3}dx + \frac{d^3\psi}{dx^2dv}dv - \frac{d^2\varepsilon}{dx^2{}_vT}\frac{dT}{T} = 0$$

or

$$\frac{d^3\psi}{dx^3}dx + \frac{d^3\psi}{dx^2dv}dv + \frac{1}{v}\frac{d^2a}{dx^2}\frac{dT}{T} = 0.$$

The curve $\frac{d^2\psi}{dx^2} = 0$ can only be found for positive value of T when $\frac{d^2a}{dx^2}$ is positive. So we derive from the latter form that $\left(\frac{dx}{dT}\right)_v$ is positive for the points for which $\frac{d^3\psi}{dx^3}$ is negative, and the other way about.

In the same way, that $\left(\frac{dv}{dT}\right)_x$ is positive for the points for which $\frac{d^3\psi}{dvdx^2}$ is negative and vice versa. The transition of the points for

which $\frac{d^3\psi}{dx^3}$ is negative or positive, takes place in the points of the curve $\frac{d^2\psi}{dx^2}=0$ with maximum or minimum volume or for which $\frac{d^3\psi}{dx^3}=0$; and the transition of the points for which $\frac{d^3\psi}{dvdx^2}=-\frac{d^2p}{dv^2_{v,T}}$ is negative or positive, takes place in the points with maximum or or minimum value of x. From all this follows that the curve $\frac{d^2\psi}{dx^2}=0$ contracts with rise of T, and has contracted to a single point for certain value of $T=T_g$. It is now necessary for our purpose to determine the value of T_g, and also the value x_g and v_g of the point at which this locus vanishes. This means analytically that we have to determine the values of T, x and v, which satisfy:

$$\frac{d^2\psi}{dx^2}=0, \qquad \frac{d^3\psi}{dx^3}=0 \quad \text{and} \quad \frac{d^3\psi}{dvdx^2}=-\frac{d^2p}{dx^2}=0$$

or the equations:

$$MRT\left\{\frac{1}{x(1-x)}+\frac{\left(\frac{db}{dx}\right)^2}{(v-b)^2}\right\}=\frac{\frac{d^2a}{dx^2}}{v} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{(1-2x)}{2x^2(1-x)^2}=\frac{\left(\frac{db}{dx}\right)^3}{(v-b)^3} \quad . \quad . \quad . \quad . \quad (2)$$

and

$$MRT\frac{\left(\frac{db}{dx}\right)^2}{(v-b)^3}=\frac{1}{2}\frac{\frac{d^2a}{dx^2}}{v^2} \quad . \quad . \quad . \quad . \quad (3)$$

If (1) is divided by (3), we get a relation between x and v, which in connection with (2) may lead to the knowledge of x_g and v_g.

Then we get:

$$\frac{(v-b)^3}{x(1-x)\left(\frac{db}{dx}\right)^2}=v+b=(v-b)+2b$$

and as $(v-b)^3=\left(\frac{db}{dx}\right)^3\frac{2x^2(1-x)^2}{1-2x}$ we find:

$$\frac{b}{\frac{db}{dx}}=\frac{x(1-x)}{1-2x}-\frac{1}{2}\left\{\frac{2x^2(1-x)^2}{1-2x}\right\}^{1/2} \quad . \quad . \quad (4)$$

and putting $b = b_1 + x\frac{db}{dx}$

$$\frac{b_1}{\frac{db}{dx}} = \frac{x^2}{1-2x} = \frac{1}{2}\left\{\frac{2x^2(1-x)^2}{1-2x}\right\}^{1/3} \quad \ldots\ldots (5)$$

The 1st member of this last equation representing the ratio between the size of the molecules of the first component and the difference of the sizes of the two kinds of molecules, we see that x_g depends only on the ratio between the sizes of the molecules of the two components.

If we take the two extreme cases 1st that b_1 may be put equal to 0, 2nd that b_2 is equal to b_1, we find the two extreme values of x_g. $\left(\frac{2x^2}{1-2x}\right)^3 = \frac{2x^2(1-x)^2}{1-2x}$ or $4x^4 = (1-x)^2(1-2x)^2$ or $2x^2 = (1-x)(1-2x)$ or $x = 1/3$ for $b_1 = 0$. For the other extreme case $\frac{db}{dx} = 0$, we find $x = 1/2$.

For some arbitrarily chosen values of x_g I have calculated the corresponding values of $\frac{b_1}{b_2 - b_1}$

x_g	$\frac{b_1}{b_2-b_1}$	$\frac{v-b}{b}$	y_g (see p. 832)
1/3	0	2	0,5
0,4	0,3704	1,115	0,358
0,45	1,5	0,505	0,216
0,46	2,08	0,457	0,186
0,47	3,06	0,363	0,154
0,48	5,04	0,265	0,117
0,49	10,91	0,191	0,0874
0,5	∞	0	0.

If on the other hand the value of x_g has been calculated by the aid of the given value of $\frac{b_1}{\frac{db}{dx}}$, v_g is determined by the aid of the equation:

$$v - b = \frac{db}{dx}\sqrt[3]{\frac{2x^2(1-x)^2}{1-2x}}.$$

If $\frac{db}{dx} = 0$, in which case $x_g = 1/2$, this equation gives an indefinite

value for $v - b$. So it is better to express $v - b$ in a value in which $\frac{db}{dx}$ does not occur. We write by the aid of formula (4):

$$(v-b) = b \frac{\sqrt[3]{\frac{2x^2(1-x)^2}{1-2x}}}{\frac{x(1-x)}{1-2x} - \frac{1}{2}\sqrt[3]{\frac{2x^2(1-x)^2}{1-2x}}}$$

or

$$\frac{v-b}{b} = \frac{2}{\sqrt[3]{\frac{4x(1-x)}{(1-2x)^2}} - 1}.$$

In the above table we find the values of $\frac{v-b}{b}$ calculated for arbitrarily chosen values of x_g. For values of x_g differing very little from $^1/_2$ the value of $\frac{v-b}{b}$ approaches $2\sqrt[3]{(1-2x_g)^2}$.

The value of MRT_g may be brought under the following form:

$$MRT_g = \frac{\frac{d^2a}{dx^2}}{b} x(1-x) \frac{1 - \left[\frac{(1-2x)^2}{4x(1-x)}\right]^{\frac{1}{3}}}{\left\{1 + \left[\frac{(1-2x)^2}{4x(1-x)}\right]^{\frac{1}{3}}\right\}^2}$$

So the temperature at which the locus $\frac{d^2\psi}{dx^2} = 0$ vanishes depends in the first place on the value of x at which it vanishes, and in the second place on the quantity $\frac{d^2a}{dx^2}$. As according to form. 5 x may lie between $\frac{1}{3}$ and $\frac{1}{2}$, the factor of $\frac{\frac{d^2a}{dx^2}}{b}$ may vary between $\frac{4}{81}$ and $\frac{1}{4}$. The value of that factor is therefore only determined by the ratio between b_1 and b_2. For $b_1 = 0$ the value is $\frac{4}{81}$, for $b_1 = b_2$ this value is $\frac{1}{4}$. So the greater the difference in the size of the molecules, the lower this factor, and the lower the temperature at which $\frac{d^2\psi}{dx^2} = 0$ has disappeared. And because the non-miscibility in the liquid state is to a great extent due to the existence of $\frac{d^2\psi}{dx^2} = 0$,

molecules of the same size $\left(\frac{d^2a}{dv^2}\right.$ being always thought equal$\left.\right)$ will not so readily mix as those for which the size of one kind greatly exceeds the size of the other kind, a property to which we might have concluded without calculations. But in the second place the quantity $\frac{d^2a}{dx^2} = 2(a_1 + a_2 - 2a_{12})$ has great influence on the height of this temperature, and indeed, in so high a degree that if $\frac{d^2a}{dx^2}$ should be $= 0$, the locus $\frac{d^2\psi}{dx^2} = 0$ would already have disappeared at the absolute zero point. Indeed, we might have seen from the very beginning that this locus could never exist for $\frac{d^2a}{dx^2}$ negative. Everything, on the other hand that diminishes a_{12}, makes T_g rise, and so furthers non-mixing. In some limiting cases we may compare the value of T_g with that of T_k, 1st in the case that into a given substance we should press a gas as 2nd component following the laws of BOYLE and GAY-LUSSAC perfectly. For such a gas we should have to put b_1 and a_1 equal to zero, and so probably also a_{12}. The value of x of the formula for T_g is then equal to $\frac{1}{3}$. The a for the mixtures containing only one term then, and being equal to a_2x^2, $\frac{d^2a}{dx^2} = 2a_2$. The value of b_x for the mixture is then equal to b_2x. On these suppositions $MRT_g = \frac{8}{27}\frac{a_2}{b_2}$, and so T_g is equal to the critical temperature of the 2nd component. The value of T_k for every mixture taken as homogeneous, is then equal to $x(T_k)_2$. Consequently $T_g = 3(T_k)_x$. For a value of T somewhat below $(T_k)_2$ the locus $\frac{d^2\psi}{dv^2} = 0$ is restricted to a very narrow region on the side of the 2nd component, while $\frac{d^2\psi}{dx^2} = 0$ still exists, and may be compared to a small circular figure whose centre is a point with the coordinates $x = {}^1/_3$ and $v = b_2$. The spinodal curve has then an equation which may be written as follows:

$$\frac{MRT}{\left(\frac{v}{x} - b_2\right)^2} = \frac{2a_2}{\left(\frac{v}{x}\right)^3},$$

which equation shows that it consists of two straight lines, which join the point $x = 0$ and $v = 0$ with the points for which $-\frac{dp}{dv} = 0$ for the second component. At temperatures which are not too far below $(T_k)_2$, the locus $\frac{d^2\psi}{dx^2} = 0$ lies, therefore, entirely outside the curve $\frac{d^2\psi}{dv^2} = 0$, and is then restricted to the left side of the figure.

2nd. As second limiting case we take $b_1 = b_2$, but a_1 differing from a_2. Then $MRT_g = \frac{d^2a}{dx^2}\frac{x(1-x)}{b}$, and because $x = \frac{1}{2}$ $MRT_g = \frac{2(a_1+a_2-2a_{12})}{4b}$, whereas MRT_k is equal to $\frac{8}{27}\frac{a_1+a_2+2a_{12}}{4b}$ for $x = {}^1/_2$. Then, too, T_g may be larger than T_k viz. when $\frac{27}{4}(a_1+a_2-2a_{12}) > (a_1+a_2+2a_{12})$ or if $2a_{12} < \frac{23}{31}(a_1+a_2)$. But even if T_g should be $< T_k$, this implies by no means that shortly before its disappearance the locus $\frac{d^2\psi}{dv^2} = 0$ lies in the region in which $\frac{d^2\psi}{dv^2}$ is negative. The previously calculated values of $\frac{v-b}{b}$ show that this disappearance takes place at a very small volume, which may be smaller, and in the limiting case will certainly be smaller than the liquid volumes of the curve $\frac{d^2\psi}{dv^2} = 0$. To ascertain whether the disappearance of $\frac{d^2\psi}{dx^2} = 0$ takes place in the region in which $\frac{d^2\psi}{dv^2}$ is negative, we may substitute the value of T_g, x_g and v_g in the form for $\frac{d^2\psi}{dv^2}$, and examine the conditions on which this value of $\frac{d^2\psi}{dv^2}$ becomes negative. If we write for $\frac{(1-2x_g)^2}{4x_g(1-x_g)} = y_g{}^3$, then:

$$MRT_g = \frac{d^2a}{dx^2}\frac{x_g(1-x_g)}{b_g}\frac{1-y_g}{(1+y_g)^2},\quad \frac{v_g-b_g}{b_g} = \frac{2y_g}{1-y_g},\quad \frac{v_g}{b_g} = \frac{1+y_g}{1-y_g}$$

and

$$\left(\frac{d^2\psi}{dv^2}\right)_g = \frac{MRT_g}{(v_g-b_g)^2} - \frac{2a}{v_g{}^2}.$$

So the sign of $\left(\frac{d^2\psi}{dv^2}\right)_g$ depends on $MRT_g - \frac{2a}{b_g}\frac{(v_g-b_g)^2}{v_g^2}\frac{b_g}{v_g}$ or on

$$\frac{d^2a}{dx^2}\frac{x_g(1-x_g)}{b_g}\frac{1-y_g}{(1+y_g)^2} - \frac{2a}{b_g}\frac{4y_g^2}{(1+y_g)^2}\frac{1-y_g}{1+y_g}$$

or on $(a_1 + a_2 - 2a_{12})\, x_g\,(1 - x_g) - a\,\frac{4y_g^2}{1+y_g}$.

For the discussion of this last quantity we first put the first mentioned limiting case, in which a_1 and a_{12} may be neglected with respect to a_2, and $a = a_2x^2$ may be put; the value of y_g being $= \frac{1}{2}$ according to the table of p. 829. With this value this quantity becomes:

$$a_2x\left\{1 - x - \frac{2}{3}x\right\}$$

so positive.

For the other limiting case for which $y_g = 0$, it is also positive. But for the intermediate cases, specially those for which $a_1 + a_2 - a_{12}$ is small with respect to a, and b_1 and b_2 are not equal, it will be negative, and shortly before its disappearance the locus $\frac{d^2\psi}{dx^2} = 0$ will lie in the region in which $\frac{d^2\psi}{dv^2}$ is negative, and the existence of this locus will have little influence on the course of the spinodal curve, and accordingly it will not give rise to a complex plait [1]) or rather to a spinodal curve which diverges greatly from the curve $\frac{d^2\psi}{dv^2} = 0$.

Let us now also examine where the point in which $\frac{d^2\psi}{dx^2} = 0$ disappears, lies with respect to the curve $\frac{d^2\psi}{dxdv} = 0$ or to $\left(\frac{dp}{dx}\right)_v = 0$. Let us substitute the value of MRT_g, v_g and v_g in the expression for $\left(\frac{dp}{dx}\right)$. If this expression becomes positive, the point lies outside the curve or rather at smaller volumes than those of the curve $\left(\frac{dp}{dx}\right)_v = 0$ and the other way about. Then we find for:

[1]) I need hardly state expressly, that in this communication I no longer attribute the cause of the complexity of the plaits exclusively to the abnormal behaviour of the components, to which at one time I thought I had to ascribe it, in compliance with LEHFELD. On the other hand it would be going too far to deny the abnormality of the components any influence.

$$MRT'_g \frac{\frac{db}{dx}}{(v_g-b)^2} - \frac{\frac{da}{dx}}{v^2}$$

$$\frac{d^2a}{dx^2}\frac{x_g(1-x_g)}{b}\frac{1-y_g}{(1+y_g)^2}\frac{db}{dx}\frac{1}{(v_g-b)^2} - \frac{\frac{da}{dx}}{v_g^2}$$

and after the substitution of $\frac{b}{\frac{db}{dx}} = \frac{x_g(1-x_g)}{1-2x_g}(1-y_g)$ and of $\left(\frac{v_g-b}{v_g}\right)^2 = \frac{4y_g^2}{(1+y_g)^2}$, the sign proves to depend on the expression:

$$\frac{d^2a}{dx^2}(1-2x_g) - \frac{da}{dx}4y_g^2.$$

In the first limiting case in which $\frac{d^2a}{dx^2} = 2a_2$, $\frac{da}{dx} = 2a_2x_g$, $y_g = \frac{1}{2}$ and $x_g = \frac{1}{3}$, this expression $= 0$. Also in the second limiting case, in which $x_g = \frac{1}{2}$ and $y_g = 0$. So in the limiting cases the curve $\frac{d^2\psi}{dxdv} = 0$ intersects the curve $\frac{d^2\psi}{dx^2}$ up to the last moment in which the latter disappears. Also in an intermediate case this quantity may be zero, but the value of x_g, at which this takes place, depends on $\frac{\frac{d^2a}{dx^2}}{\frac{da}{dx}}$ or on $\frac{(a_2-a_{12})-(a_{12}-a_1)}{(a_{12}-a_1)(1-x_g)+x_g(a_2-a_{12})}$.

If we write $\frac{a_2-a_{12}}{a_{12}-a_2} = 1+\Delta$, then $\frac{\frac{d^2a}{dx^2}}{\frac{da}{dx}} = \frac{\Delta}{1-x_g+x_g(1+\Delta)} = \frac{\Delta}{1+x_g\Delta}$. We then derive the value of x_g for this intermediate case from the equation:

$$\frac{\Delta}{1+\Delta x_g}(1-2x_g) = 4\frac{(1-2x_g)^{4/3}}{[4x_g(1-x_g)]^{2/3}}$$

or

$$\frac{\Delta}{1+\Delta x_g} = 4\left[\frac{1-2x_g}{16x_g^2(1-x_g)^2}\right]^{1/3}.$$

For values of x_g differing little from $\frac{1}{2}$ we find approximately:

$$\frac{1}{2} - x_g = \frac{1}{128}\left(\frac{\Delta}{1+\frac{1}{2}\Delta}\right)^3.$$

If for Δ we take the value 4, which must be considered large for molecules of about the same size, then $\frac{1}{2} - x_g$ would be $= \frac{1}{54}$. The conclusion which we draw from all this as to the situation of the point in which $\frac{d^2\psi}{dv^2} = 0$ disappears, with respect to the curve $\frac{d^2\psi}{dxdv} = 0$, is the following. In most cases this point disappears within the curve $\left(\frac{dp}{dx}\right)_v = 0$, and so in the region where $\left(\frac{dp}{dx}\right)_v$ is negative, but this can also take place on the other side of $\left(\frac{dp}{dx}\right)_v = 0$, so at a volume which is smaller than that of this curve.

That at positive value of Δ, so at positive value of $\frac{a_1+a_2-2a_{12}}{a_{12}-a_1}$ $\frac{\Delta}{1+x\Delta} = 4^{1/3}\frac{(1-2x)^{1/3}}{x^{2/3}(1-x)^{2/3}}$ has always a root, appears immediately when we represent the two members of this equation graphically. The first member, namely, represents then a branch of a hyperbola which at $x = 0$ has a height above the axis of x equal to that of Δ, and at $x = 1$ a height equal to $\frac{\Delta}{1+\Delta}$, and which, therefore, proceeds continuously at a certain positive though decreasing height above the x-axis. The second member represents a line which for $x = 0$ has a point infinitely far above, and for $x = 1$ a point infinitely far below the x-axis. This line passes through the point $x = \frac{1}{2}$, and on the left and the right of this point the ordinates are equal, but with reversed sign. So intersection will certainly take place, and for positive Δ at a value of $x < \frac{1}{2}$. For the case that $\frac{d^2\psi}{dx^2} = 0$ disappears at smaller volume than that of the curve $\frac{dp}{dx} = 0$, the first member must be larger than the second. As Δ is larger, the point of intersection will be further removed from $x = \frac{1}{2}$, and so the series of the values of x for which the condition is that the first member be larger

than the second member, has increased. From this we conclude that $\frac{d^2\psi}{dx^2} = 0$ may disappear also for very different size of the molecules in the region for $\frac{dp}{dx} =$ positive, if Δ has a considerable size. But for perfectly unequal size of the molecules $\left(x = \frac{1}{3}\right)$, $\frac{\Delta}{1+\frac{\Delta}{3}}$ would be > 3 or $\frac{\Delta}{3+\Delta} > 1$, which is not yet satisfied even at $\Delta = \infty$.

Fig. 6, in which the intersection of $\left(\frac{dp}{dx}\right)_v = 0$ and $\frac{d^2\psi}{dx^2} = 0$ has been drawn in both points on the left of the point in which $\left(\frac{dp}{dx}\right)_v = 0$ has the minimum volume, holds for this latter case. The point in which $\frac{d^2\psi}{dx^2} = 0$ disappears, must viz., lie on the line $\frac{d^2p}{dx^2} = 0$. As has already been mentioned before, this line passes through the point where $\left(\frac{dp}{dx}\right)_v = 0$ has its smallest volume, and as is easy to calculate $\frac{dv}{dx}$ is then always positive. If now in fig. 6 the line $\frac{d^2\psi}{dx^2} = 0$ contracts, and it must vanish on $\frac{d^2p}{dx^2} = 0$, then the point in which it disappears, lies at smaller volume than that of $\left(\frac{dp}{dx}\right)_v = 0$. For the opposite case the two points of intersection must therefore be drawn right of the point with minimum volume. Also the intermediate case has now become clear. In this respect there is an inaccuracy in the drawing of fig. 5. The line $\frac{d^2\psi}{dx^2} = 0$, which has already almost quite contracted, must be expected there on the right of the point in which $\left(\frac{dp}{dx}\right)_v = 0$ has the smallest volume. So the line $\left(\frac{dp}{dx}\right)_v = 0$ would have its minimum volume more to the left in fig. 5. In fact, with rise of temperature all these lines are subjected to displacements — however, not to such a degree that the relative position is much changed by it.

All these remarks seem essential to me for the following reason: we shall, namely, soon have to draw the relative position of the curves $\frac{d^2\psi}{dx^2} = 0$ and $\frac{d^2\psi}{dv^2} = 0$, also in regions lying more to the right of fig. 1, in order to decide about the more or less complexity of the plaits at the different temperatures. Then we shall have to make assumptions as to this relative position, which otherwise might seem quite unjustifiable. A great many more similar questions would even have to be put and solved, before alle doubt as to the validity of the assumptions would have been removed. And it remains the question if for the present the imperfect knowledge of the equation of state for small volumes does not prevent our ascertaining with perfect certainty whether a phenomenon of mixing or non-mixing is either normal or abnormal. So, before proceeding to the applications I shall subject only one more point to a closer investigation, viz. the question whether in the critical point of a mixture taken as homogeneous, the quantity $\frac{d^2\psi}{dx^2}$ is positive or negative, so the sign of the quantity:

$$\frac{8}{27}\frac{a}{b}\left\{\frac{1}{x(1-x)} + \frac{\left(\frac{db}{dx}\right)^2}{4b^2}\right\} - \frac{\frac{d^2a}{dx^2}}{3b}$$

or of

$$\frac{1}{x(1-x)} + \frac{\left(\frac{db}{dx}\right)^2}{4b^2} - \frac{9}{8}\frac{\frac{d^2a}{dx^2}}{a}.$$

As $2a\frac{d^2a}{dx^2} = \left(\frac{da}{dx}\right)^2 + 4(a_1 a_2 - a_{12}^2)$, we may also write for the last form:

$$\frac{1}{x(1-x)} + \frac{\left(\frac{db}{dx}\right)^2}{4b^2} - \frac{9}{16}\frac{\left(\frac{da}{dx}\right)^2}{a^2} - \frac{9}{4}\frac{a_1 a_2 - a_{12}^2}{a^2}.$$

As a first special case we consider a substance mixed with a perfect gas; then $b_1 = 0$, $a_1 = 0$ and $a_{12} = 0$. Hence $a = a_2 x^2$, $b = b_2 x$. With these values the above form becomes:

$$\frac{1}{x(1-x)} - \frac{2}{x^2} = \frac{3x-2}{x^2(1-x)}$$

so $\frac{d^2\psi}{dx^2}$ is negative in the critical point for values of $x < {}^2/_3$; for $x = {}^2/_3$ the curve $\frac{d^2\psi}{dx^2} = 0$ will pass through the critical point. But

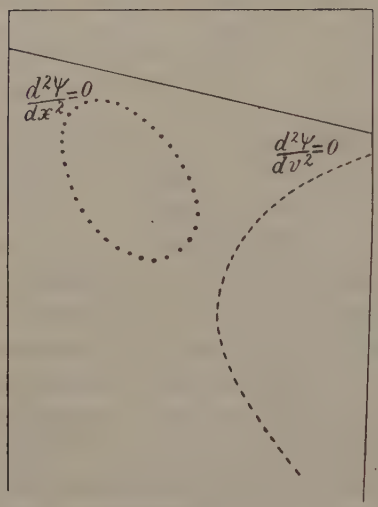


Fig. 10.

for $x > {}^2/_3$ the two curves will lie outside each other, as has been drawn in fig. 10, and already observed above. For all other cases in which a and b cannot be equal to zero, the value of the expression for $x=0$ and $x=1$ will be positive infinite. If it can become negative, this will, accordingly, have to be the case for two values of x. Now very different relations may exist between $\frac{1}{a}\frac{da}{dx}$ and $\frac{1}{b}\frac{db}{dx}$. Thus $\frac{1}{a}\frac{da}{dx} = {}^2/_3 \frac{1}{b}\frac{db}{dx}$ for the plaitpoint circumstances of a mixture taken as homogeneous. [1]) With these values the form reduces to a quantity which will certainly be positive, as even if $a_1 a_2 > a_{12}^2$, the value of $\frac{a_1 a_2 - a_{12}^2}{a^2}$ can probably never be larger than $\frac{4}{9}\frac{1}{x(1-x)}$, the minimum value of which is $\frac{16}{9}$.

If $\frac{1}{b}\frac{db}{dx} = \frac{1}{a}\frac{da}{dx}$, the sign of the form under discussion, depends on the value of $\frac{da}{dx}$ or $\frac{1}{b}\frac{db}{dx}$. If $\frac{1}{b}\frac{db}{dx} > \sqrt{\frac{16}{5}\frac{1}{x(1-x)}\frac{1}{a}}$, a negative value of the form is possible. So for mixtures, in which the components differ greatly in the size of the molecules, the case of fig. 11 occurs for minimum T_k, and this minimum value of T_k could not be realised. For mixtures, for which $\frac{1}{a}\frac{da}{dx} > \frac{1}{b}\frac{db}{dx}$, $\left(\frac{1}{b}\frac{db}{dx}\right)^2$ may be neglected with respect to $\frac{9}{4}\left(\frac{1}{a}\frac{da}{dx}\right)^4$, which is even perfectly allowable in the limiting case, for which $b_1 = b_2$, and $\frac{d^2\psi}{dx^2}$ will be negative,

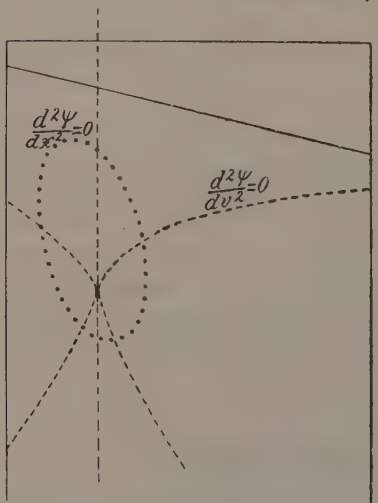


Fig. 11.

[1] In all the above calculations the equation of state has been applied with

when we put:

$$\frac{3}{4}\left(\frac{1}{a}\frac{da}{dx}\right) > \sqrt{\frac{1}{x(1-x)}}.$$

As minimum value for which this is the case, we should then have:

$$\frac{1}{a}\frac{da}{dx} > \frac{8}{3}$$

In all such cases, in which the critical circumstances of a mixture taken as homogeneous, fall in the region in which $\frac{d^2\psi}{dx^2} < 0$, these circumstances are not to be realised. Nor are they to be realised when $\frac{d^2\psi}{dx^2} > 0$, but then the spinodal curve passes at least at a small distance round this point, and the plaitpoint circumstances are not very different from these which would be the critical circumstances with an homogeneous substance. If $\frac{d^2\psi}{dx^2} < 0$, a considerable difference may be expected.

THE SPINODAL CURVE AND THE PLAITPOINTS WHEN $\frac{dT_k}{dx}$ IS POSITIVE.

Let us now again proceed to the discussion of the course of the spinodal curve and the plaitpoints; but now in the case that with increasing value of b the quantity T_k rises. Let T_{k_2} be much higher than T_{k_1}, and $\frac{a_2}{b_2} > \frac{a_1}{b_1}$. Now two cases are possible. The value of $\frac{d^2\psi}{dx^2}$ may be positive or negative in the critical points of every arbitrary mixture. For $x = 0$, and in general for very small value of x, where $\frac{1}{x(1-x)}$ is very large, $\frac{d^2\psi}{dx^2}$ is certainly positive, however large the value of $\frac{d^2a}{dx^2}$: a may be, and also for values of x differing little from 1. If $\frac{\frac{d^2a}{dx^2}}{a}$ is small, $\frac{d^2\psi}{dx^2}$ is positive in the critical points of all mixtures. But for large values of $\frac{d^2a}{dx^2}$: a there are two values of x, between

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value of b not depending on v. Hence in this equation we get the factor $^2/_3$ for which, as we have already repeatedly observed, we should really substitute $\frac{5}{6}$.

which $\frac{d^2\psi}{dx^2}$ is negative in the critical points. If in this case we draw the line $\frac{d^2\psi}{dv^2} = 0$ with a top, either at $x = 0$, or at a small value of x, the curve $\frac{d^2\psi}{dx^2} = 0$, which is chiefly restricted to the left side of the v,x-figure, lies partly outside the curve $\frac{d^2\psi}{dv^2} = 0$, and on the small volumes. If we now apply the reasoning of p. 737 etc. also in this case, when we had the reversed state as far as $\frac{d^2\psi}{dv^2} = 0$ is concerned, we conclude that for large values of x the spinodal curve does not move for away from $\frac{d^2\psi}{dv^2} = 0$, but that it is forced back to smaller volumes for those values of x, where $\frac{d^2\psi}{dx^2}$ is negative, and draws again very near to $\frac{d^2\psi}{dv^2} = 0$ with very small values of x. Naturally the course of the q-lines in connection with the course of the p-lines must indicate this.

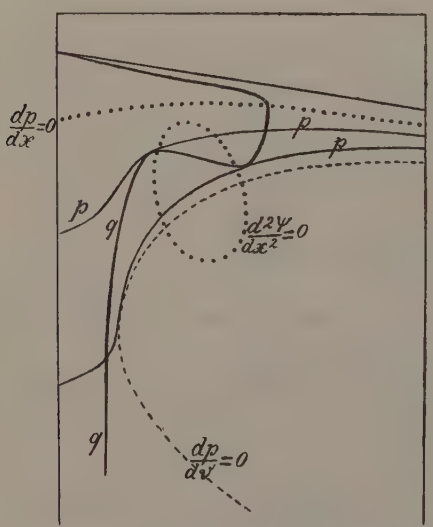


Fig. 12.

The course of the p-lines for this case must be derived from the right side of fig. 1, from which appears at the same time that the curve $\frac{d^2\psi}{dxdv} = 0$ occurs, but with sensibly smaller volumes than those of $\frac{d^2\psi}{dv^2} = 0$. And the course of the q-lines is then indicated by fig. 5 or perhaps sometimes by fig. 6. In fig. 12 a couple of p-lines have been drawn and a q-line which touches these p-lines, which lines yield, therefore, points for the spinodal curve. Here again three plaitpoints are to be expected: 1st. a realisable plaitpoint P_1 above the curve $\frac{d^2\psi}{dx^2} = 0$, 2nd. a hidden plaitpoint P_2 on the left of $\frac{d^2\psi}{dx^2} = 0$ and above $\frac{d^2\psi}{dv^2} = 0$, and 3rd. the ordinary gas-liquid plait-

point P_3 on the left of $\frac{d^2\psi}{dv^2} = 0$, but shifted to the side of the small volumes. Now it is to be expected that the value of p in the first mentioned plaitpoint is smaller than in the last mentioned. For T_k strongly rising, the pressure strongly decreases when we pass along the curve $\frac{d^2\psi}{dv^2}$ to the right — and only if $\frac{d^2\psi}{dx^2} = 0$ should strongly project above $\frac{d^2\psi}{dv^2} = 0$ we should enter the region of high pressures. The hidden plaitpoint has, of course, far lower pressure than the two others. The value of x for the first mentioned plaitpoint is larger than that for the hidden plaitpoint. The gas-liquid plaitpoint has the smallest value of x. Proceeding along the spinodal curve we get a course of p, as has been previously drawn by me. (See These Proc. March 1905 p. 626). If T is made to increase, $\frac{d^2\psi}{dv^2} = 0$ contracts. The top moves to the right, and reaches a position, in which $\frac{d^2\psi}{dx^2}$ is negative for the critical circumstances. But this means that the gas-liquid plaitpoint and the hidden plaitpoint have coincided already before. When they coincide we have again, as we observed p. 744 $\left(\frac{dv}{dx}\right)_p = \left(\frac{dv}{dx}\right)_q$, $\left(\frac{d^2v}{dx^2}\right)_p = \left(\frac{d^2v}{dx^2}\right)_q$ and $\left(\frac{d^3v}{dx^3}\right)_p = \left(\frac{d^3v}{dx^3}\right)_q$. After this coinciding we have again a simple plait with a simple plaitpoint. But the plaitpoint lies far more to the left than would be the case if the curve $\frac{d^2\psi}{dx^2} = 0$ did not exist any longer, and it also has a much larger pressure. With further rise of T nothing of importance is to be expected. For neither the fact that $\frac{d^2\psi}{dx^2} = 0$ lies quite outside $\frac{d^2\psi}{dv^2} = 0$, nor that $\frac{d^2\psi}{dx^2} = 0$ vanishes, gives rise to new phenomena, because this takes entirely place in the unstable region. If we now draw either the value of the plaitpoint temperature or of the plaitpoint pressure as function of x, and if we restrict ourselves to the realisable quantities, so excluding the hidden ones, this line separates into two detached parts. The right part begins at that value of x, in which the plaitpoint P_1 possesses a pressure large enough to show itself on the binodal curve of the plait whose plaitpoint is P_3, and passes then to $x = 1$. The left part begins at $x = 0$, and disappears before P_3 and P_2 coincide, namely, when P_3 lies on the binodal line, of which P_1 is then the plaitpoint.

That what we have called hidden plaitpoints, can never exhibit themselves, requires no explanation. That what in general we have called realisable plaitpoints, need not always show themselves, may indeed be assumed as known from the former thermodynamic considerations about the properties of the ψ-surface — but yet it calls for further elucidation now that we examine the properties of stability and of realisability by considering the relative position of the p- and the q-lines. We shall, however, only be able to give this elucidation, when by treating the rule to which I alluded in the beginning of this communication, we have also indicated the construction of the binodal curve.

To get a clear insight into the critical phenomena for the case that for mixtures between two definite values of x the critical point falls in the region in which $\frac{d^2\psi}{dx^2}$ is negative, we must again distinguish two cases; viz.: 1. the case that already at $T = T_{k_1}$ the curve $\frac{d^2\psi}{dx^2}$ partly projects above $\frac{d^2\psi}{dv^2} = 0$, in which case already at $T = T_{k_1}$ the two plaitpoints P_1 and P_2 are found, and 2. the case that at $T = T_{k_1}$ the curve $\frac{d^2\psi}{dx^2} = 0$, lies quite enclosed within $\frac{d^2\psi}{dv^2} = 0$. In fig. 13 the second case is represented. Now if for values of $T > T_{k_1}$ the top of $\frac{dp}{dv} = 0$ does fall within $\frac{d^2\psi}{dx^2} = 0$ there must have been contact of the two curves at a lower T, and intersection at a higher T. As long as the two curves do not yet touch, the spinodal curve is little or not transformed, and no other plaitpoint is to be expected as yet than the ordinary gas-liquid plaitpoint which lies at smaller value of x than the top of $\frac{dp}{dv} = 0$. If the two curves intersect, the plaitpoints P_1 and P_2 have appeared first as coinciding heterogeneous plaitpoints, later as two separate ones. Naturally the

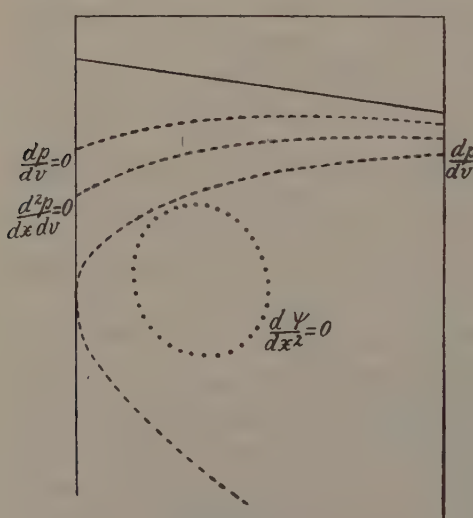


Fig. 13.

value of x for the two coinciding heterogeneous plaitpoints is larger than the value of x for the plaitpoint P_3. With further rise of the temperature, when $\frac{d^2\psi}{dx^2} = 0$ rises further above $\frac{d^2\psi}{dv^2} = 0$, the plaitpoints P_1 and P_2 move further apart. P_1 moves towards larger values of x, and P_2 (the hidden plaitpoints) to smaller value of x. And the two heterogeneous plaitpoints P_3 and P_2 coinciding at still higher value of T, there is a continuous series of values of x from $x = 0$ up to $x = 1$, for which plaitpoints occur. For every value of x only one. I have drawn (These Proc. VII, p. 626) the transformation of what I called there a principal plait and a branch plait. But this transformation refers, properly speaking, more to the binodal curve of such a complex plait than to the spinodal curve. If we then draw T_{pl} as function of x, such a line has a maximum and a minimum value, both lying above T_{k_1}. The minimum value at the origin of the double plaitpoint P_1 and P_2, and the maximum value at the disappearance of P_2 and P_3 in consequence of their coinciding. Also when P_{pl} is drawn as function of x, we get a similar curve. As in general $\frac{dp}{dx} = \frac{dp}{dx_T} + \left(\frac{dp}{dT}\right)_x \frac{dT}{dx}$, $\frac{dT_{pl}}{dx}$ will be $= 0$, if $\frac{dP_{pl}}{dx} = 0$, because $\left(\frac{dp}{dx}\right)_T$ is equal to 0 in a plaitpoint. But the value of P_{pl} as function of T_{pl} exhibits a more intricate form. As $T\frac{dP_{pl}}{dT} = T\left(\frac{dp}{dT}\right)_{v,x} + \frac{(-)}{\left(\frac{d^2v}{dx^2}\right)_{pT}}$, $\frac{dP_{pl}}{dT}$ is determined by the properties of the substance in the plaitpoint, e. g. by $\frac{d^2v}{dx^2}_{pT}$. This quantity is the same for double plaitpoint, and so $\frac{dP_{pl}}{dT}$ has two equal values in such a double plaitpoint. The plaitpoint curve has therefore two cusps in the case treated. The left branch extends from T_{k_1} to the temperature at which P_3 and P_2 coincide. The right branch begins at T_{k_2}, and runs then back to the temperature at which the double plaitpoint P_1 and P_2 originates. The middle branch gives the hidden plaitpoints. But here, too, we must again notice that not the whole outside branches can actually be realised, the splitting up into three phases when we draw near the cusps having a greater stability then the homogeneous plaitpoint phase. These are the phenomena observed by KUENEN for the mixtures of ethane and alcohols with greater values of b than that of ethane. Perhaps the change of

$\left(\frac{db}{dx}\right)$ may already account for the fact that the peculiar feature of this phenomenon disappears more and more when, retaining ethane, we choose an alcohol with larger value of b; so that the phenomena point to the fact that a normal plaitpoint line might be expected if we proceed in the series of the alcohols. As condition for $\frac{d^2\psi}{dx^2}$ being negative in the critical circumstances, we had:

$$\frac{1}{x(1-x)} + \frac{\left(\frac{db}{dx}\right)^2}{4b^2} - \frac{9}{8}\frac{\frac{d^2a}{dx^2}}{a} < 0 \quad \text{(see p. 837)}$$

For in general it is to be expected that this value cannot so easily be realised for large value of $\frac{db}{dx}$ than e. g. for almost equal value of b_1 and b_2. That the mixture of ethane with methyl alcohol displayed quite different phenomena might already be expected on account of the fact that we have then a case for which with increasing value of b the value of T_k decreases. It is viz. almost sure that b is smaller for methyl alcohol than for ethane.

If for $T = T_{k_1}$ the curve $\frac{d^2\psi}{dx^2} = 0$ should already partially project above $\frac{d^2\psi}{dx^2} = 0$, this will bring about but little change in the phenomenon. Only the minimum value of T_{pl} will descend below T_{k_1} in the (T_{pl}, x)-figure. In the same way the left cusp will have to be drawn at lower value of T than T_{k_1}.

It is, therefore, required for the course of the plaitpoint phenomena, that $T_g > T_{k_1}$, and so according to the value of T_g (p. 830).

$$\frac{\frac{d^2a}{dx^2}}{b} x(1-x)\frac{(1-y_g)}{(1+y_g)^3} > \frac{8}{27}\frac{a_1}{b_1}.$$

In this inequality x, dependent on the value of $\frac{b_1}{b_2 - b_1}$, lies between $\frac{1}{3}$ and $\frac{1}{2}$, and y_g between $\frac{1}{2}$ and 0. Let us write:

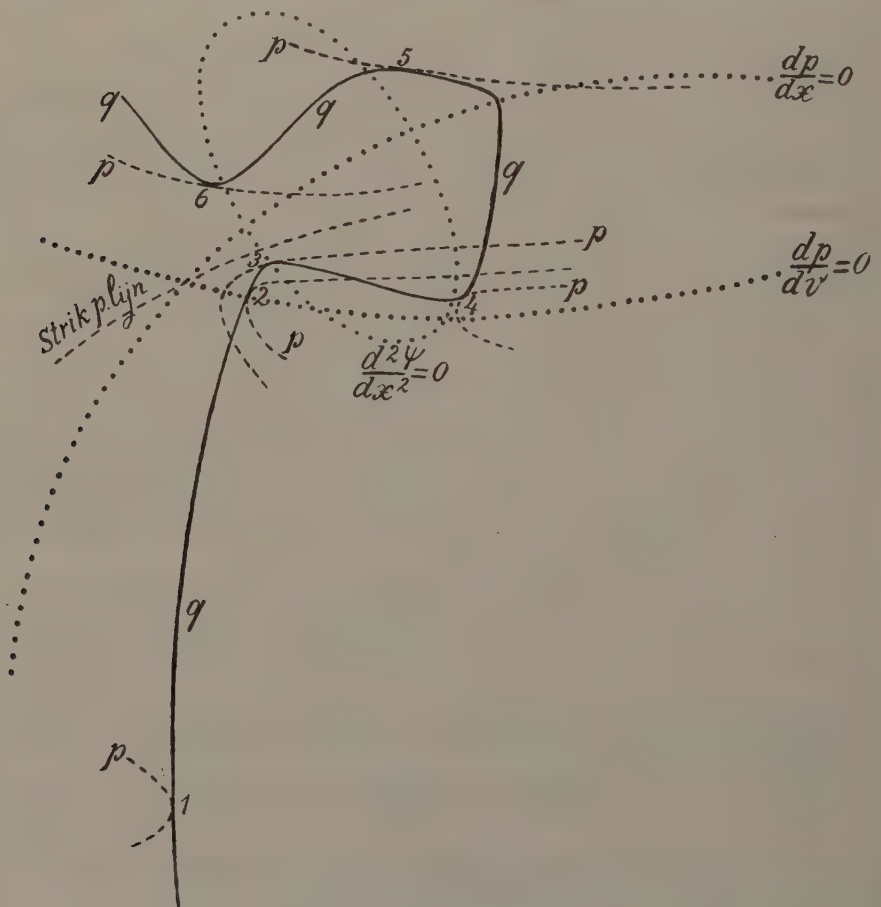
$$\frac{a_1 + a_2 - 2a_{12}}{a_1} x(1-x)\frac{(1-y_g)}{(1+y_g)^3} > \frac{4}{27}\left\{1 + \frac{x}{\frac{b_1}{b_2 - b_1}}\right\}.$$

By successively increasing x from $\frac{1}{3}$ to $\frac{1}{2}$, and deriving the corresponding values y_g and $\frac{b_1}{b_2-b_1}$ from the table of p. 829, we can calculate the value which $\frac{a_1+a_2-2a_{12}}{a_1}$ must have at the least in order to satisfy this inequality. If we put $x=\frac{1}{3}$, to which $\frac{b_1}{b_2-b_1}=0$ corresponds, we see that only $a_1=0$ might be put. If x is made to increase, which implies that the ratio of the size of the molecules approaches 1, the value of $\frac{a_1+a_2-2a_{12}}{a_1}$ required to satisfy the inequality, decreases. For the limiting case $x=\frac{1}{2}$, $b_1=b_2$ and $y_g=0$, $\frac{a_1+a_2-2a_{12}}{a_1}$ must be $>\frac{16}{27}$ to enable us to put $T_g>T_{k_1}$.

But this value must be larger for b_1 smaller than b_2, and the larger as the difference between b_2 and b_1 increases. If this equality is not satisfied, so if $T_g<T_{k_1}$, we have a plaitpoint line of a perfectly normal shape. This is inter alia the case when for a low ratio between b_1 and b_2, also a not very high ratio between the critical temperatures is found. First, however, we should have to know how a_{12} depends on a_1 and a_2, before for given ratio of b_1 and b_2 we could indicate how large the ratio of $\frac{a_1}{b_1}$ and $\frac{a_2}{b_2}$ would have to be to justify us in expecting either the complicated or the simple shape of the plaitpoint line. Moreover, I repeat that it should be considered in how far numerical values occurring in the given equations, would have to be replaced by others on account of the only approximate validity of the equation of state.

From all this appears in how high a degree the properties of the function $\frac{d^2\psi}{dx^2}$ influence the shape of the plait, and so also the miscibility or non-miscibility in the liquid state, and that the influence of the properties of this function may be put on a level with that of the function $\frac{d^2\psi}{dv^2}$. We shall further demonstrate this by also examining the case that the curve $\frac{d^2\psi}{dx^2}=0$ exists, and intersects the curve $\frac{d^2\psi}{dxdv}=0$.

Let us now take a region of fig. 1 such that the line $\frac{d^2\psi}{dxdv} = 0$ occurs on it, and that this line has the position as drawn in fig. 6. Then the liquid branch of $\frac{d^2\psi}{dv^2} = 0$ lies on the right side of the region at larger volumes than those of $\frac{d^2\psi}{dxdv} = 0$. These two curves might intersect on the left side. If now also the curve $\frac{d^2\psi}{dx^2} = 0$ occurs, which will be the case if the temperature is low enough, and if this curve intersects both $\frac{d^2\psi}{dxdv} = 0$ and $\frac{d^2\psi}{dv^2} = 0$, we have the shape of the q-lines as drawn in fig. 6, and there will again be formed a complex plait, whose shape and properties we shall have to examine.



Strik-p-lijn = loop-p-line.
Fig. 14.

That intersection may take place of $\frac{d^2\psi}{dx^2} = 0$ with $\frac{d^2\psi}{dxdv} = 0$, has been proved on pages 834 and 835.

We saw before that one q-line may possess 2 or 4 points of contact with p-lines, but now we have a case in which the number of points of contact can rise to 6. In fig. 14 has been drawn: 1. the curve $\frac{dp}{dx} = 0$ and $\frac{dp}{dv} = 0$, 2. the loop-p-line, 3. a q-line to which horizontal tangents may be drawn in 4 points, and a vertical tangent in 1 point and 4 portions of 6 p-lines touching the q-line. The pressure in point 1 is much larger than in 2, rises then, has a maximum in 3, descends again and reaches in 4 its lowest value. The greatest pressure is found in point 5, and in 6 the pressure has been drawn lower than in 5, but it may be higher than in point 1. Consult fig. 1 for the direction of the p-lines in the points of contact. These 6 points of contact are again points of the spinodal curve. So on the right there is again a portion of the spinodal curve which follows closely the line $\frac{dp}{dv} = 0$ in its course, also on the left a portion that does not move far away from this line. But between these two portions the spinodal curve must have been strongly forced back towards smaller volumes to avoid the line $\frac{d^2\psi}{dx^2} = 0$.

In the points where $\frac{dp}{dx} = 0$ intersects the curve $\frac{d^2\psi}{dx^2} = 0$ the spinodal curve touches this curve, because $\frac{d^2\psi}{dx^2}$ must be $= \frac{\left(\frac{d^2\psi}{dxdv}\right)^2}{\frac{d^2\psi}{dv^2}}$ for the points of the spinodal curve, and so it must remain in the region where $\frac{d^2\psi}{dx^2}$ is positive, except when $\frac{d^2\psi}{dxdv} = 0$. It may then even be doubted if $v > b$ is found for all the points of the spinodal curve.

Values of $v < b$ would mean that the left part and the right part of the liquid branch of the spinodal line would remain separated from each other; and this would imply for the miscibility or non-miscibility of the components that at the temperatures for which this is the case, even infinitely large pressure would be insufficient to bring about mixing. Already in my Théorie Moléculaire I raised this problem, and I showed, that if b is a linear function of x, cases

are conceivable in which the spinodal line could intersect the line $v = b$ twice, but that if $\frac{d^2b}{dx^2}$ has positive value, as is really to be expected, intersection will never take place. But if we acknowledge again that the knowledge of the equation of state is insufficient for very small volumes it follows that we had better not pronounce the solution of this question too decisively.

If the spinodal line is closed on the side of the small volumes, then a realisable plaitpoint will be found there, while there must be a hidden plaitpoint in the neighbourhood of the points 2 and 3. If the temperature is raised, the line $\frac{d^2\psi}{dx^2} = 0$, as it now also intersects the line $\frac{d^2\psi}{dxdv} = 0$, can contract to above $\frac{d^2\psi}{dv^2} = 0$, before disappearing. If it has sufficiently ascended above $\frac{d^2\psi}{dv^2} = 0$, the spinodal line will get a point where it splits [1]) up, at which 2 new plaitpoints (homogeneous ones) are formed. So at this splitting point $\frac{d^2v}{dx^2{}_p} = 0$ and $\frac{d^3v}{dx^2{}_q} = 0$. This furnishes an indication as to the place where this splitting point will lie. That the q-line below $\frac{d^2\psi}{dxdv} = 0$ must have a point of inflection, has been shown before (p. 736), where we derived a series of points of inflection of the q-lines passing through the point in which $\frac{d^2\psi}{dx^2} = 0$ has the greatest volume. We have also previously (p. 628) derived a series of points of inflection of the p-lines starting from the point where $\frac{dp}{dx} = 0$ and $\frac{dp}{dv} = 0$ intersect, and passing through the point where $\frac{dp}{dx} = 0$ has the minimum volume. From this we conclude that the double plaitpoint can only occur when the line $\frac{dp}{dx} = 0$ is intersected by $\frac{d^2\psi}{dx^2} = 0$ pretty far to the left of the point with minimum volume, and so not far to the right of the asymptote of the line $\frac{dp}{dx} = 0$.

[1]) This splitting point I had already in view in my Théorie Moléculaire (Cont. II p. 42 and 43) where I indicate the temperature at which the detached plait (longitudinal plait) leaves the v,x-diagram, when it has not contracted to a single point.

I may remark in passing that VAN DER LEE's observations for water and phenol illustrate the case discussed here, and that through the existence of a maximum pressure the properties of the vapour-liquid binodal line give evidence either of the occurrence of the asymptote of the line $\frac{dp}{dx} = 0$ in the v,x-diagram, or of its lying not far to the left. So there are 4 plaitpoints after the appearance of this double plaitpoint. So two serve as plaitpoints of the plait which is detaching itself and they are both realisable according to our nomenclature and when detachment has taken place, both can actually be realised. They serve then as plaitpoints of what must properly be called a *longitudinal plait*. The two other plaitpoints, viz. the hidden plaitpoint which we placed in the neighbourhood of the points 2 and 3 above, and the lowest of the newly formed plaitpoints then form a couple of heterogeneous plaitpoints, which do not show themselves on the binodal curve of the vapour-liquid plait and will soon coincide and then disappear. From this moment the binodal lines of the two plaits are quite separated and behave independently of each other. The vapour-liquid plait is then simple and perfectly normal. But also the longitudinal plait may then be considered as a normal one.

(*To be continued.*)

Waterstaat. — "*Velocities of the current in an open Panama canal.*" By Dr. C. LELY.

(Communicated in the meeting of March 30, 1907).

§ 1. After an elaborate investigation the American Government has resolved on the execution of a project of a Panamacanal at high level, *viz.* at a height of 85 feet (25.9 M.) above the mean sea level. It will have three flights of locks.

Against this project of the minority of the Board of Consulting Engineers of 1905 there was a counterproject of the majority which favoured a canal at sea-level or rather a canal with one pair of locks. This canal would have been provided with one pair of locks in order to separate the Atlantic Ocean from the Pacific, but for the rest it would have been in open communication with these seas on both sides of the locks.

As a matter of fact this canal would not have been an open canal, therefore, like the Suez Canal, but a canal in which in most cases,

if not in all, lockage would be necessary. A canal, therefore, which probably would have resembled more closely to the lockcanal proposed for Suez but not executed and strongly opposed, than to the present open Suez-Canal.

The question therefore presents itself whether the Panama-Canal, like the Suez-Canal might not have been made *open* and *without sluices.*

The technical commission of the International Congress of Paris in 1879 deemed a lock near the Panama-terminal an absolute necessity, because it was supposed that, without it the tidal motion of the Pacific would cause currents in the canal of a velocity of 2 to 2.50 M. per second [1]).

On the other hand the Board of Consulting Engineers of 1905 rightly judged that the necessity of such a lock was not established but, owing to lack of time, it was not able to investigate the matter [2]).

On page 56 of the report we find as follows:

"The question of the necessity of a tidal lock at the Panama end "of the canal has been raised by engineers of repute, but the limited "time available to the Board has not permitted the full consideration "of this question which is desirable. It is probable that in the "absence of a tidal lock the tidal currents during extreme spring "oscillations would reach five miles per hour. "(2.24 M. per second)" "While it might be possible to devise facilities which would permit "ships of large size to enter or leave the canal during the existence

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[1]) This opinion clashed with that of the original projectors Messrs Wijse and Reclus. In a statement made by the latter at the meeting of the Technical Commission of May 19, 1879 he explains that the inclination of the high and low waterlines in the Panama-Canal will be about the same as on the Suez-Canal, as a consequence whereof velocities of the current might be expected in the Panama-Canal which would not exceed very appreciably those of the Suez-Canal. The latter, as far as they are due solely to the tides, usually do not exceed 0.90 M. per second; under the influence of wind they may increase to 1.30 or 1.35 M.

[2]) At the time of the meeting of the Consulting Board competent experts were still of opinion that a lock at the Pacific-terminal would be necessary. Such appears clearly from the letter of Mr. T. P. Shonts Chairman of the Isthmian Canal Commission received by the Board at the beginning of its labours. In this letter occur the following lines:

"A disadvantage which the two plans have in common is that the rapid develop-"ments of naval architecture make it difficult to determine the proper dimensions "of the lock chambers. It is to be considered, however, that up to the present "time such developments has not been greatly hampered by deficient depth in the "harbors of the world, and that development here after will have that obstruction "to contend with. Moreover, it is not possible to dispense with locks entirely. Even "with the sea-level canal a tide lock will be required at the Panama end".

"of such currents, the Board has considered it advisable to contem-
"plate and estimate for twin tidal locks located near Sosa Hill
'even though the period during which they would be needed would
"probably be confined to a part of each spring tide."

It would require a special investigation, however, to know whether in a canal provided with locks, those locks would have to be used only during part of the spring tides.

For, the oscillations of the sea above and below the mean level executed in a period of three hours are on an average ± 1.23 M. at neap tide and ± 2.53 M. at spring time. This being so it seems probable enough that, both in the interest of navigation and to prevent eventual damages which might be caused by the closing of the lockgates against a strong current, lockage of the ships would be preferred to passing the lock with gates open. For, assuming the total profile of the locks to be equal to the profile of the canal, observations made in the Suez-Canal justify us in evaluating the velocity of the current at 0.70 to 0.90 M. at mean neap tide and at 1.00 to 1.30 M. at mean spring tide.

At all events, each time after the gates having been closed the passing of the lock with gates open would not be possible before the sea had again reached its mean level. As a consequence, at each tide requiring the closing of the gates, the period during which passing of the lock with open gates would be possible, would be less than three of the six hours included between two returns of the sea to its mean level.

Howsoever this be and leaving out of consideration the question to what degree a lock in a sea level canal will be an obstacle to navigation, it appears at all events that the necessity af such a lock has remained an unsolved question when in 1905 the projects of a Panamacanal were examined. The cause thereof lies in the uncertainty about the velocity of the currents which will occur in an open canal, particularly as a consequence of the tidal motion of the Pacific.

In addition to the motion caused by the tides, great velocities of the current may occur in a sea level-canal, with or without tidal lock, at the time of high floods of the Chagres and other rivers, if the water of these rivers must be carried off by the canal. In contradistinction to the project of 1879 such would have been the case in the sea-level canal according to the project of the Board of Consulting Engineers.

The Board comes to the conclusion that in a sea level canal with tidal lock currents will thus be caused reaching a maximum

velocity of 1.18 M. per second. (2.64 miles per hour). The Board is of opinion that such a velocity will be no hindrance to navigation.

These same velocities will occur in an open canal as well as in a sea-level canal with tidal lock, at least if in both cases the water of the rivers must be carried off by the canal. They occur very rarely however and need not necessarily lead to an increase of the maximum velocities caused by the tidal motion.

§ 2. The reasons which led the technical commission of the Congress of Paris in 1879 to expect currents with a velocity of 2—2,50 M. per second in an open canal, are twofold. In the first place, the commission gave some examples of currents with a velocity of 2—3,50 M. per second observed on the lower course of rivers where similar differences exist between high and low-water as on the Panama canal on the Side of the Pacific.[1])

In the second place the commission published a memorandum of Mr. KLEITZ, one of its members, containing some summary calculations in regard to the velocities which must be expected in an open canal.[2])

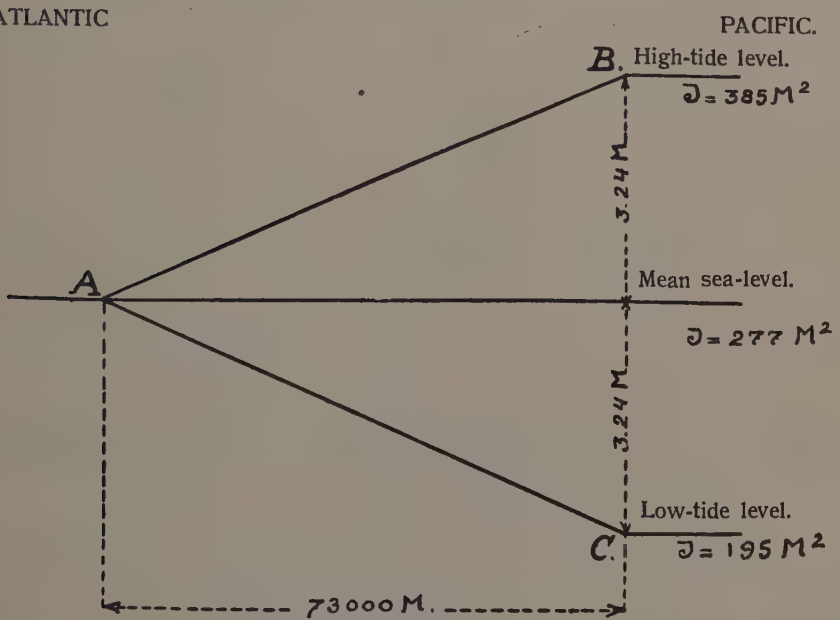
It is evident that on the lower end of a river with a great amplitude of the tide very considerable velocities of the current *may* occur; but it does not follow that equal velocities *will* occur in an open Panama-canal. This will be the case only if the remaining circumstances which have a decisive influence on the velocity are about the same in the two cases. Now it is evident that the velocity of the current caused by the tidal motion of the water will be no less dependent on the depth and in particular on the mean depth for the whole of the width, than on the amplitude of the tide and this irrespective of the question whether we have to do with a river or with an open canal of relatively great length. In other words the velocity of the current will depend as well on the proportion of the amplitude to the mean depth as on each of these quantities separately. Furthermore it is easily seen that in a river these velocities will depend in a great measure not only on the discharge but also on the changes of width and depth and on the inclination of the bottom near its mouth. In fact, the examples communicated by the commission show clearly that the velocity must be dependent in a high measure on other causes besides the amplitude of the tides. For among the examples of the commission we find the Rivière de l'Odet with an amplitude of the tides of 5 M. and

[1]) See: Congrès international d'études du canal interocéanique. Compte rendu des séances. Paris 1879 page 362.

[2]) Ib. p. 384 and Pl. IV fig. 6.

velocity of the high water flow of 3.50 M. and furthermore La Charente with an amplitude of the tides of 6.35 M. and a velocity of the high-water flow of 2 M.

As regards the calculation of Mr. KLEITZ, it is as follows:



According to the above figure the area of the wet section of the canal on the side of the Pacific was adopted to be 385 M.² at high-water and 195 M.² at low water.

The difference between the mass of water in the canal at high and low tide is then taken for the volume of the prism A B C.

$$\text{therefore} = 73000 \times \frac{385 - 195}{2}\ \text{M}^3 = 6.935.000\ \text{M}^3.$$

As the interval between high and low tide is about six hours, the change of the mass of water per second is found to be 321 M³ on an average. The mean wet section of the canal on the side of the Pacific being $^1/_2$ (385 + 195) M², that is 290 M², Mr. KLEITZ derives for the velocity of the inflow during the whole period of high tide or for the outflow during the whole period of low tide

$$\frac{321}{290} = 1.11\ \text{M}.$$

Furthermore, assuming that the most rapid change of the mass of the water will occur about at the time at which the sea-level is

equal to the mean level and besides, that this most rapid change is equal to double the mean change, the maximum inflow is put at $2 \times 321 = 642$ M³.

As the wet section of the canal at the mean level is about 277 M², we find $\frac{642}{272} = 2.32$ M. per second for the maximum velocity.

It is easily seen that these calculations are valueless. For the fact has been wholly overlooked that a certain time must elapse before some rise or fall at the mouth of the canal on the Pacific will make itself felt over the whole length of the canal. If therefore, shortly after ebb, the level in the canal near its mouth begins to rise and, shortly afterwards, the first inflow takes place, the level of the canal further inland will still be falling and the water will there be flowing out as a consequence. Similarly when shortly after the moment of high tide on the sea, the level of the canal near its mouth begins to fall and shortly afterwards outflow sets in, the level further inland will still be rising and there the inflow will not yet have ceased.

Moreover the in- and outflow of the canal on the side of the Atlantic has been left wholly out of consideration. They will certainly not be small but will not take place at the same moments as the in- and outflow on the side of the Pacific. We may see that the difference in time, before mentioned, will not be insignificant but will have a great importance, by considering that, on the Suez-canal, the propagation of the high tide takes place with a velocity of about 10 M. p. second. Assuming the same velocity for the Panama-canal the propagation of the tidal motion over the whole of the length of the canal will require about 2 hours. As a consequence the currents near the two terminals of the canal will have different directions during a great part of the tide.

The incorrectness of the reasons for the conclusion of the congress of 1879, according to which a lock is to be considered an absolute necessity seems to have attracted little attention at that time, and consequently the canal was originally executed with the intention of building a sluice on the side of the Pacific.

FERDINAND DE LESSEPS, who always considered it a great advantage that the Suez-canal was executed without locks, probably never favoured this lock in the project of the Panama-canal. This led him in May 1886 to address himself to the French Academy of Sciences, requesting it to institute an investigation about the influence of the tidal motion of the Pacific and the Atlantic on the motion of the water in an open Panama-canal.

The commission charged with this investigation reported on the

matter in the meeting of 31 May 1887. This commission consisted of the members of the section of Geography and Navigation and besides of the members DAUBRÉE, FAVÉ LALANNE, DE JONQUIÈRES and BOUSSINESQ and the reporter BOUQUET DE LA GRIJE.

This report, though short, contains the results of extensive computations, which led the commission to the following highly remarkable and important conclusion.

"*que, dans aucun cas, les courants dus à la dénivellation ne pour-*
"*ront depasser* $2\frac{1}{2}$ *noeud*" (± 1.29 *M. par seconde,* "*et que cette vitesse,*
"*qui ne peut être atteinte tous les ans que pendant quelques heures,*
"*ne parait pas de nature à gêner la navigation des bateaux à vapeur*
"*dans le canal que l'on creuse actuellement à Panama*".

This conclusion was accepted by the Academy and the question concerning the possibility of an open Panama-canal without locks was placed in quite another light than that in which it appeared after the congress of 1879.

Owing to particular circumstances, this conclusion of the French Academy of Sciences has attracted comparatively little attention. For in the same year that this conclusion was reached, the original project of a sea-level canal with lock had to be given up and to be replaced by a canal with several locks. It was the beginning of the sufferings of the Panama-canal.

Since then the principal consideration has always been to limit the excavations to the utmost. For this purpose the hilly country required a canal at high level, consequently several locks.

§ 3. Therefore, if we wish to answer the question whether an open Panama-canal without sluices is possible, we have to inquire in the first place, whether the report of the French Academy of Sciences, of 1887 is based on sound foundations.

What were these foundations?

In accordance with observations at the tide-gauge at Panama the differences between high and low water, in other words, the amplitudes of the tides at the mouth of the Panama-canal were adopted to amount to:

at neap tide, on an average	2.46 M.
„ spring „ „ „ „	5.06 M.
„ „ „ maximum in March or Sept.	6.76 M.

The commission now calculates the velocity of the current for this maximum difference in height of the tides on the Pacific of 6.76 M.,

neglecting the usually small tidal oscillation in the Atlantic and further starting from the following suppositions:

1. that experience shows that on a canal communicating on the one side with a sea of variable level, on the other side with a sea of constant level, the amplitude of the tidal curve diminishes uniformly from one sea to the other and further that the retardation of the tide is proportional to the distance, that therefore:

if $Y =$ half the amplitude of the tides of the Pacific,
$l =$ length of the canal,
$\omega =$ velocity of propagation of the tides,

the level y, with respect to the mean canal- or sea-level, at a distance x from the Pacific, will be:

$$y = - Y\left(1 - \frac{x}{l}\right) \cos.\left(2t - \frac{x}{\omega}\right)$$

2. that, in accordance with what has been observed on similar canals, particularly on the Suez-canal between Suez and the Bitter-Lakes, the velocity of propagation of the tide can be represented by the well known formula:

$$\omega = \sqrt{g\left(H + \frac{3}{2}y\right)} \pm Kv$$

where:

$H =$ depth of the canal below mean sea-level,
$v =$ velocity of the current,
$K =$ constant (0.4 at flood-time, 1.2 at ebb);

3. that, from the levels which have been derived by means of the suppositions 1. and 2. for any moment and for two mutually not too distant places, the velocity of the current for that moment may be computed by applying the formula:

$$v = 56{,}86\sqrt{Ri} - 0.07.$$

By means of these suppositions the velocity of the currents have been computed for places at 9, 27, 45 and 63 K.M. from the Pacific, assuming a tide of the amplitude of 6.76 M. The results are as follows [1]):

---

[1]) The length of the canal which according to the project made at that time, would amount to 72 K.M. has been put at 76 K.M. in the calculations to allow for the curves. The bottomwidth was put at 21 M, the depth at 11.50 M. below mean sea-level at Panama, and 9 M. at Colon, the slopes at 1 horizontal on 1 vertical.

Time elapsed since low tide on the Pacific.	Distances from the Pacific.			
	9 K.M.	27 K.M.	45 K.M.	63 K.M.
Moon-hours.	Velocities of the current in M. per second.			
0	— 0.95	— 1.00	— 0.77	— 0.69
½	— 0.81	— 0.90	— 0.93	— 0.72
1	— 0.60	— 0.84	— 0.87	— 0.83
1½	— 0.13	— 0.75	— 0.82	— 0.85
2	+ 0.35	— 0.59	— 0.75	— 0.86
2½	+ 0.67	— 0.34	— 0.63	— 0.81
3	+ 0.84	+ 0.35	— 0.42	— 0.73
3½	+ 0.93	+ 0.63	+ 0.08	— 0.61
4	+ 0.98	+ 0.78	+ 0.43	— 0.41
4½	+ 1.02	+ 0.93	+ 0.80	0
5	+ 1.17	+ 1.06	+ 0.82	+ 0.51
5½	+ 1.16	+ 1.11	+ 0.86	+ 0.66
6	+ 1.09	+ 1.06	+ 0.98	+ 0.76
6½	+ 0.97	+ 1.01	+ 0.97	+ 0.85

+ = current *from* the Pacific *towards* the Atlantic
— = „ „ „ Atlantic „ „ Pacific

From these computations follows that the maximum velocity in the canal on the side of the Pacific, due exclusively to the tidal motion, will amount to 1.17 M. Supposing that there might be some difference between the mean sea level of the Atlantic and the Pacific and that this difference might amount to 0.50 M., the commission concludes that the maximum velocity might then increase to 1.26 M. The commission thus finally arrives at the conclusion referred to above.

§ 4. The two first suppositions on which the computations are based will probably not seriously deviate from the truth. For they are, at least partially, confirmed by what is observed on the Suez-canal.

The commission further points out, that the formula for the velocity of propagation of the tidal wave, which has been derived in the supposition that the amplitude of the tide is relatively small as compared with the depth of the water, leads to results which, for the Suez-canal, agree closely with the observations. For the formula

leads to a velocity of propagation of 10.06 M., whereas we find 9.54 M. by observation.

Matters stand somewhat differently for the third supposition. The formula by which the velocities of the currents are computed is the well known formula for permanent uniform motion. It is in the nature of the thing that such a motion cannot occur in a canal where a strong tidal motion takes place. But the question on which every thing depends is not so much this, whether the use of this formula leads to sufficiently correct velocities for any moment, as the following, whether the computed maximum velocities are not too small.

In reference to this question we may remark that in general the formula will lead to too small a value of the velocity during the period that change in level is accompanied by decrease of inclination; to too great a value where the change is accompanied by an increase of inclination.

If, taking this into consideration, we examine the parts of the canal K.M. 0—9 and K.M. 9—27, during the period of $4^1/_2$ to 6 hours after low tide on the Pacific, we get as follows:

Time elapsed since low tide	Mean inclination	
	K.M. 0—9	K.M. 9—27
$4^1/_2$ hours	0.000044	0.000040
5 „	0.000048	0.000046
$5^1/_2$ „	0.000048	0.000047
6 „	0.000044	0.000045

From these data it appears that, during the half hour preceding the epochs at which the velocities reach their maximum value at K.M. 9 and 27 the mean inclination for the part 0—9 as well as for the part 9—27 has been little variable but increasing.

From this it follows that by the application of the formula at these epochs we probably cannot have made any important error.

Meanwhile, in order to test the validity of the computations, we have still to inquire whether the computed velocities, taken in conjunction with the computed levels, satisfy the equation of continuity.

$$\frac{dI}{dt} = -I\frac{dv}{dx} - v\frac{dI}{dx}$$

where I represents the area, v the mean velocity of the wet section at the distance x from the Pacific, at the epoch t.

We can make out, approximately, in how far the computed levels and the velocities satisfy this condition by availing ourselves of the levels and velocities computed for each half hour and for the different distances from the Pacific. We thus find as follows:

A. For the differences in the discharge at 9 *and* 27 *K.M. distance from the Pacific.*

Moon-hours elapsed since low tide on the Pacific.	Area *I*		Velocity *v*		Discharge *Iv*		Per half hour in part 9—27		Excess of *in*flow over *out*flow in half an hour
	9	27	9	27	9	27	*in* flow	*out* flow	
	M².	M².	M.	M.	M³.	M³.	M³.	M³.	M³.
4½	450	388	1.02	0.93	459	361	914000	713000	+201000
5	475	407	1.17	1.06	556	431	1.013000	807000	+206000
5½	491	420	1.16	1.11	570	466	1.002000	828000	+174000
6	498	428	1.09	1.06	543	454			

B. For the change of the mass of water contained in part 9—27.

Moon-hours elapsed since low tide on the Pacific.	Area *I*		Change of area during half an hour.		Mean change for part 9—27.	Change of mass per half hour for part 9—27.
	9	27	9	27		
	M².	M².	M².	M².	M².	M³.
4½	450	388				
			+ 25	+ 19	+ 22	+ 396000
5	475	407				
			+ 16	+ 13	+ 14⁵	+ 261000
5½	491	420				
			+ 7	+ 8	+ 7⁵	+ 135000
6	498	428				

Comparing the last columns of the tables A and B we get the following differences for part 9—27:

from 4½ to 5 hours + 195000 M³, or on an average per sec. + 108 M³.
„ 5 „ 5½ „ + 55000 „ „ „ „ „ „ „ + 31 „
„ 5½ „ 6 „ — 39000 „ „ „ „ „ „ „ — 22 „

It appears from this comparison, that by the computed velocities, taken in conjunction with the computed levels, the condition of continuity is not fully satisfied.

Therefore, assuming the levels to be correct, the velocities need some correction.

Suppose these corrections for the consecutive half hours to be

$$\text{for K.M. } 9 = \delta_1, \delta_2, \delta_3, \delta_4,$$
$$\text{for K.M. } 27 = \delta_1', \delta_2', \delta_3', \delta_4',$$

we find for the values of the corrections:

$\delta_1 = +0.15$ *M.*	$\delta_1' = -0{,}13$ *M.*
$\delta_2 = +0.12$ „	$\delta_2' = -0{,}10$ „
$\delta_3 = -0.04$ „	$\delta_3' = +0{,}04$ „
$\delta_4 = -0{,}01$ „	$\delta_4' = +0{,}01$ „

Therefore, applying the corrections, for the velocities themselves:

	at K.M. 9	at K.M. 27
at $4^1/_2$ hours	1,17 M.	0,80 M.
„ 5 „	1,29 „	0,96 „
„ $5^1/_2$ „	1,12 „	1,15 „
„ 6 „	1,08 „	1,07 „

From these numbers it appears that we can satisfy the condition of continuity at least for the part 9—27, during the period between $4^1/_2$ and 6 hours after low tide, by relatively speaking slight modifications of the computed velocities.

It cannot be denied, however, that the circumstance of the condition of continuity not being necessarily satisfied in applying this method of computing the velocities, indicates that this method is uncertain to some extent; though it appears that the uncertainty, at least as regards the calculation of the maximum velocities, will be small.

Another reason of uncertainty in the computation of the velocities lies in the value assumed for the coefficient of the formula for uniform motion.

This value, 56,86, is not the result of a great number of observations made on rivers and canals of about the same inclination and depth as the Panama-canal, but of observations for rivers of considerably smaller depth.

We may of course test the validity of this coefficient, as well as, more generally, the validity of the formula itself, by comparing the velocities it yields for the Suez-canal with those really observed there. Of the observations which have been made about the velocities in that part of the canal which lies between the Bitter Lakes and Port-Thewfik, those of 23 July, and 8 en 22 August and 6 September 1892 have been published [1]).

These observations, however, are insufficient for a fair comparison.

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[1]) See: The Suez-canal according to the posthumous papers of I. F. W. Conrad arranged by R. A. van Sandick. Tijdschrift Kon. Instituut van Ingenieurs 1902—1903, p. 89 and 90.

They have been made for two parts of the canal each 200 M. in length and separated by only 4.9 K.M. One part was included in that division of the canal which at that time had been widened to a bottomwidth of 37 M. while the other, having a bottomwidth of only 22 M., was situated a little beyond the point of transition to the not yet widened canal. As a consequence the motion of the water on the whole of this part of the canal, 4.9 K.M. in length, cannot have been uniform [1]).

Moreover these observations are only relative to the velocities in the middle of the current, observed by means of floats down to a depth of 6 M. below the surface, whereas the velocity given by the formula represents the average velocity for the whole of the wet section. Meanwhile a comparison of these observations with the results obtained by the formula might still give some idea about the reliability of the formula.

The comparison of the observations referred to above with the results yielded by the formula, putting the coefficient at 56.86, lead to the following results:

OBSERVATIONS ON THE SUEZ-CANAL IN 1892.

Day and hour of the observation	Direction of the current.	Distance between the places of observation.	Mean difference of level between the places of observation.	Observed velocities. Averages during an hour in the		Computed mean velocity for the widened part. [2])
				widened part	unwidened part	
		K.M.	M.	M.	M.	M.
23 July 11—12 a.m.	flood	4.9	+ 0.12	+ 0.75	+ 0.97	+ 0.64
„ „ 5—6 p.m.	ebb	4.9	— 0.14	— 0.84	— 1.11	— 0.58
8 Aug. 11—12 a.m.	flood	4.9	+ 0.09	+ 0.69	+ 0.87	+ 0.47
„ „ 5—6 p.m.	ebb	4.9	— 0.11	— 0.80	— 0.93	— 0.57
22 „ 6—7 a.m.	ebb	4.9	— 0.16	— 0.88	— 1.05	— 0.68
„ „ 12—1 p.m.	flood	4.9	+ 0.07	+ 0.66	+ 0.82	+ 0.46
6 Sept. 11—12 a.m.	flood	4.9	+ 0.07	+ 0 66	+ 0 89	+ 0.47
„ „ 5—6 p.m.	ebb	4.9	— 0.10	— 0.85	— 0.98	— 0.53

[1]) The first part was the widened part of the canal between K.M. 149 and 149.2; the other the not widened part between K.M. 144.1 and 144.3. The transition of the widened to the not widened part was situated at K.M. 144.4.

[2]) As the part of the canal from K.M. 149 to 144.4 had been widened the observed difference of level is relative to the widened part.

From this table we derive for the proportion between the computed average velocity for the whole of the wet section to the velocities observed down to 6 M. in the middle of the widened part of the canal, the following values:

at high water flow (from the Red Sea)	at ebb flow (towards the Red Sea)
1.17	1.45
1.47	1.40
1.43	1.30
1.40	1.61
Mean 1.37	1.44

The true value of this proportion for the case in which observation and computation agree, is unknown. But if we consider that the floats went down to only 6 M. below the surface, whereas the depth of the water at flood tide was over 8.50 M. and at ebb time over 7.50 M. and furthermore, that the canal had side slopes of 1 vertical on $2^1/_2$ horizontal, we conclude that at all events the velocity in the middle must have considerably exceeded the average velocity for the whole of the section. As far as can be ascertained therefore, the formula applied to the Suez-Canal leads to results which do not clash with the observation.

More conclusive information cannot be derived from a comparison of the computed velocities to the observed values. As long therefore as complete observations, made for the widened Suez canal, concerning the relation between the velocity of the current, the tidal motion and the dimentions of the section, have not furnished us with more reliable information about the value of the coefficient and about the question whether the formula applies fully to the case, we cannot avoid a relatively considerable uncertainty in the calculation of the maximum velocity.

§ 5. A closer examination is therefore required to decide in how far the velocity of the current in an open canal may cause a hindrance to navigation and whether this hindrance cannot be overcome.

In discussing this question we must consider, on the one hand that the computed velocities represent average velocities for the whole of the wet section and that therefore the absolute velocities in the middle of the canal will be more considerable; on the other hand, however, that the computed velocities are relative to the greatest possible differences in the height of the tide. The computed maximum

velocities may occur therefore only on a couple of days every year. And on these days only during a few hours.

In how far a relatively rare velocity of the current offers difficulties to navigation is of course ascertained in the best way by a comparison to canals on which under similar conditions similar velocities occur. For such a comparison the Suez-canal offers the best conditions. For this canal several observations about the velocity of the current are known. Published observations, however, cannot lay claim to completeness, at least not for the present purpose. In the first place because they have not been frequent enough to justify the belief that among them will have occurred these rare cases which by an unfavourable coincidence of circumstances, must have given rise to exceptionally great velocities. In the second place because the measurements are, as a rule, relative to absolute velocities in the middle of the canal and not to the average velocity for the whole of the wet section.

Moreover, in comparing the Panama-canal to the Suez-canal we have to consider that the dimensions of the former will be much more considerable than those of the latter as originally executed. Consequently such velocities as have caused no difficulties for the Suez-canal will cause them still less for the Panama-canal.

For the Suez-canal between the Bitter Lakes and Suez originally had a bottomwidth of 22 M. and a depth of 8 M. below mean springtide low water, with which dimensions corresponds a cross section of 330 M^2. On the other hand the sea level Panama-canal would get a bottomwidth of about 45.7 M. (150 feet) and a depth of about 12.2 M. (40 feet) corresponding with a cross section of 855 M^2.

Observations, made during the period 1871—1876, have brought to light the following facts about the velocities of the current in the Suez-canal between the Bitter Lakes and Suez. [1])

"The maximum velocity of the high water flow, running North-"ward, amounts to 0.80 to 0.90 M. at the springtides of the months "of May and November, to 1.15—1.35 M. p. s. in the months of "January and February.

"The maximum velocity of the ebb flow running Southward amounts "to 0.75—0.80 M. at the springtides of the months of May and Novem-"ber, to 1.20—1.25 M. p. s. in the months of July and August.

"Along Port-Thewfik in the canal south of the main channel

1) *Vide* the paper of Mr. J. F. W. Conrad pp. 89 and 90.

"towards Suez, bottomwidth 80 M., the velocity of the high water "flow at springtide is 0.60 to 0.70 M., at neaptide 0.45 to 0.50 M., "in the winter-season with strong South wind 1.00 to 1.20 M. p. s. "The velocity of the ebb flow at springtide is 0.55 to 0.60 M. In "the summer with strong North wind 0.90 M. p. s.

"Outside the mouth of the canal at Port-Thewfik no velocity of "the current has been observed."

The observations of 23 July 1892 made under circumstances which, as regards the flow, were certainly not unfavourable, led already to velocities which, at flood tide, ranged from 0.95—1.03 M. and were in the mean 0.97 M. at flood tide and 1.11 M. at ebb.

Mr. Dauzats, chief engineer of the Suez-canal, speaking at the meetings of the Technical subcommission of the International Congress for the Panama canal in 1879, stated in regard to the sidings of the Suez-canal, as follows [1]):

"Dans les canaux où le courant est faible, et là où n'existe aucun courant, il suffit de faire les gares d'un seul côté ; mais dès que la vitesse atteint 0.75 ou 1.50 M, il faut les établir des deux côtés et en face l'une de l'autre".

By this statement we are certainly justified in concluding that the said engineer, founding his opinion on his experience of the Suez-canal, deemed allowable velocities of the current of 1.50 M. The small original bottomwidth of the Suez-canal of 22 M., however, caused difficulties for the simultaneous navigation in both directions.

The following communications of Mr. E. Quellennec, consulting engineer of the Suez-canal company, proves that these velocities of the current offer no difficulties even for the big ships which at present navigate the Suez-canal. These communications to the Board of Consulting Engineers of 1905 are as follows :

"In the Suez section the velocity of the current very often exceeds 0.60 meter per second, and reaches at times 1.35 Meters per second.

"In the latter case the ships do not steer very well with the "current running in ; however the navigation is never interrupted "on account of the current. In the Port Said section ships can "moor with a current running in either direction ; in the Suez "section they always moor with the current running out". [2])

The canal between the Bitter Lakes and the Red Sea has at present a width of about 37 M., but a widening of the cross section

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[1]) See: Congrès international etc 1879, p. 361.

[2]) See: Report of the Board of Consulting Engineers for the Panama-canal, Washington 1906, p. 176.

to 45 M. width and 10.5 M. depth is being executed. After this widening, navigation will certainly experience still less difficulty than at present. Meanwhile, and this point deserves attention, the velocity of the current after the completion of the widening for the whole of the canal between Suez and the Bitter Lakes, will not be lessened but increased. For, owing to the surface of the two Bitter Lakes, which is about 23800 H.A., the widening will only cause insignificant modifications in the level of these Lakes. Consequently the fall of the water between the Red Sea and the Bitter Lakes will be nearly unaltered after the widening both at high — and low water. Under these circumstances the enlargement of the cross section will necessarily cause increased velocity of the current.

The mere consideration of the maximum velocity which may occur during a few hours every year, and even then only on the side of the Pacific, is evidently inadequate for reaching a true estimate about the question whether the velocities of the current in an open Panama-canal without lock will offer difficulties of any importance for navigation. We have to pay regard in the first place to the velocities which will regularly occur on the whole length of the canal at mean spring-tide and mean neaptide.

These velocities may be derived with some approximation from those found by the French Academy for a maximum difference in tide of 6.76 M.[1]), at least if we suppose that these velocities will not considerably deviate from the truth.

We thus find for the *maximum velocities*

	at K.M.	9	27	45	63
at mean	neap tide:	0.70 M.	0.67 M.	0.59 M.	0.51 M.
,, ,,	spring ,,	1.01 ,,	0.96 ,,	0.85 ,,	0.74 ,,

The following diagrams show the velocities of the current, for the interval of from 9 to 63 K.M. distance to the Pacific, at mean spring tide and mean neap tide, 0 to 6 Moon-hours after ebb on the Pacific. They were derived from the calculations of the French Academy of Sciences.

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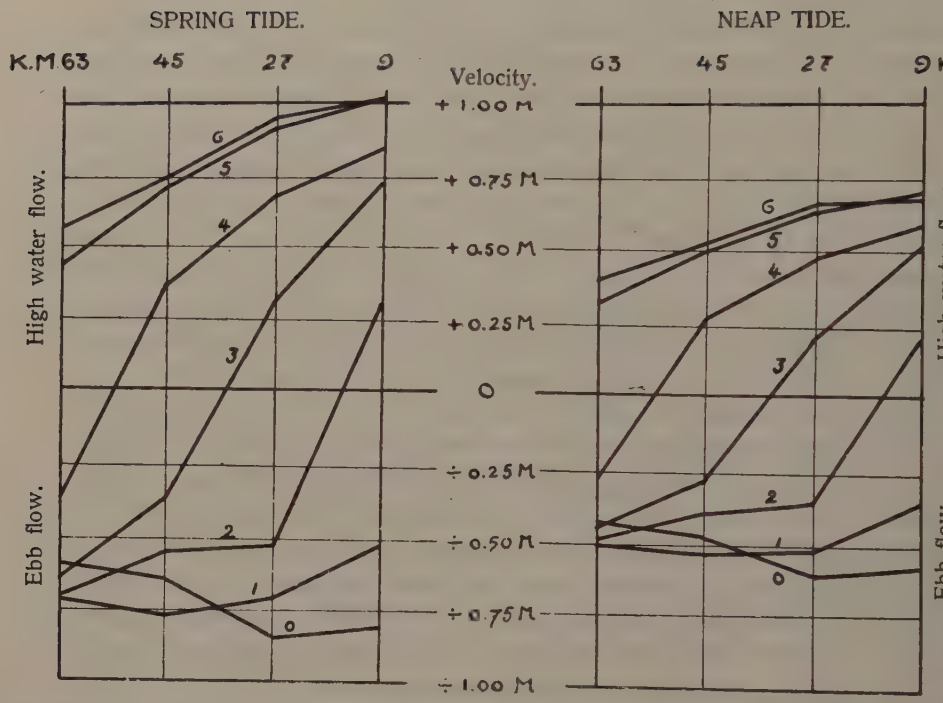
[1]) The approximation neglects the differences of the velocities of propagation of the tide for different amplitudes We thus obtain for the velocity v', at an arbitrary place, the amplitude being y', the following value, which is expressed in terms of the velocity v for an amplitude y:

$$(v' + 0.07) = (v + 0.07)\sqrt{\frac{y'}{y}}$$



The figures inscribed in the diagram represent the hours elapsed since low tide in the Pacific
+ = current *from* the Pacific towards the Atlantic
÷ = „ „ „ Atlantic „ „ Pacific.

§ 6. From the preceding considerations we may conclude that, as far as we can judge by direct computation of the velocities, to be expected in an open Panama canal, there is reason to think that these velocities will indeed be *somewhat*, but not considerably greater than those on the Suez-canal between the Bitter Lakes and the Red Sea.

Meanwhile we ought not to forget, that both in these computations and in our knowledge about the velocities which occur on the Suez-canal there remains some or rather considerable uncertainty. This uncertainty might only be diminished by more complete observations than have been published as yet concerning the relation between velocity of the current, tidal motion and dimensions of the cross-section of the Suez-canal.

We shall be enabled to get at a just estimate therefore about the question whether an open Panama-canal without lock is possible, only by following a way different from that of a comparison of the computed velocities with those observed on the Suez-canal. This way may consist in trying to get at a direct knowledge of the *differences* of the velocities on the two canals by a comparison of the circum-

stances which will occur on the two. Afterwards the circumstance that, on the Suez-canal the velocity of the current offers no difficulty, in conjunction with the probable value of the velocity of this canal, will help us in deciding whether these differences are of such a nature as to produce undoubted difficulties on the Panama-canal.

In making this comparison it will be permissible to assume that the violent winds occurring in the Suez-canal, which cause velocities of the current 0.30 to 0.50 M. in excess of those due to the tidal motions, are not to be expected on the Panama-canal near the Pacific.

First, however, we have to inquire whether an open canal cannot be executed in such a way that for that part where the current will be greatest the difficulties caused by such great velocities can be removed. It is evident that this would be possible only by giving a very great width to the canal. This is practically impossible for that part of the canal which intersects mountainous country, but it is well feasible for that part of the canal which extends from the Pacific to the Culebra mountain, that is to near Pedro-Miguel, a part which for the greater part intersects low country.

If to this part of the canal, where just the greatest velocities will occur, a bottomwidth is given of for instance 500 feet (about 150 M.) instead of 150 feet (45.7 M.) no difficulties will be experienced from any presumable velocity of the current.

Such a widening of the canal on the side of the Pacific would however increase the inclination and the velocity of the current in the remaining part, at least if no particular measures are taken to prevent such increase.

These measures would necessarily consist in making a reservoir or lake in open communication with the widened part of the canal. This reservoir or lake would have to be of such an area that it would be capable of retaining the water which, during the rise of the level, it would receive from the widened part in excess of what would be discharged by the unwidened part. During the fall of the level it would restore this surplus to the widened part.

From the nature of the thing this arrangement is theoretically possible. Whether it be practically possible depends on the surface which a determinate widening would entail.

A lake of somewhat over 800 H.A., such as is represented on Plate I, is feasible in the low country bordering on the canal near its mouth on the Pacific.

Starting from this area it is possible to determine the degree of widening which may be given to the part near the Pacific in such a way that, under given circumstances, for instance at spring tide,

no change will occur in the gradient of the high and low water lines, nor in the velocity of the current in the remaining part of the canal.

As soon as the amplitude of the tides exceeds that of springtide the inclination and the velocity of the current will be somewhat increased for the wider part, somewhat diminished for the remaining part, as compared with what they would be without the widening of the first part and without the addition of a lake. In the case of a smaller amplitude of the tides the reverse will occur.

Owing to the situation of the ground the junction of the widened canal with the lake must be made at a distance of about 12 K.M. from the Pacific terminal of the canal. Not before 3 K.M. farther however, that is not before 15 K.M. from the sea, the surface of the lake reaches a considerable breadth. Therefore if the inclination of the high and low water lines remains nearly unchanged and if, according to the most recent project, the length of the canal is fixed at about 80 K.M., the amplitude of the tide in the lake may reach $(5.06 - 15 \times 0.0632)$ M. $= \pm$ **4.10** M.

With such an amplitude a mass of water may be received, in the interval between high and low water, of $800 \times 10.000 \times 4.10$ M³. $= 32{,}800{,}000$ M³.

Assuming, as an approximation, that this mass is received within a period of six hours, we find that *on an average* 1500 M³. will be received per second.

The surplus width of the part of the canal near the Pacific must be determined in such a way, therefore, that *on an average* 1500 M³. may be displaced — without increase of the velocity of the current — in excess of what might be displaced if the width remained normal.

It is not well possible, without elaborate computation, to fix accurately the surplus width necessary for the purpose. But it is easily seen that this *surplus* width must be about 100 M. so that a bottomwidth of 150 M. might be given to the widened part extending from the entrance of the canal to the junction with the lake. Corresponding therewith the width at the spring tide level would be about 250 M. At K.M. 64 this width might gradually be reduced to the normal width.

It will be possible therefore to remove eventual difficulties offered by considerable velocity of the current on the part of the canal nearest the Pacific, by increasing the bottomwidth of this part. (16 K. M. in length).

Now let us consider how the case stands for the remaining part of the canal, 64 K. M. in length.

On this part the inclination of the high and low water lines will amount to 3.16 cM. at mean springtide and to 1.52 cM. at mean neaptide.

On the Suez-canal the inclination of the high and low water lines between the Bitter Lakes and the Red Sea amounts to 2.52 cM. per. K. M. at mean spring tide and to 1.48 cM. at mean neaptide.

Under the influence of the direction and force of the wind, the height of the tides on the Suez-canal may be increased or diminished by about 0.25—0,33 M.

As a consequence the inclination of the high and low water lines may be increased by about 1 cM. per K. M.

As the distance of the Bitter Lakes to the Red Sea is about 28 K.M., this already enables us to conclude that the velocities of the current in an open Panama-canal, for the first 28 K. M. on the side of the Atlantic, cannot greatly differ from those which occur on the Suez-canal (See Plate II).

If therefore — leaving out of consideration the absolute value of the velocities — we may assume that the velocity of the current will offer no difficulties on the Suez-canal even when it will have been widened, then it follows that on an open Panama-canal, for about the distance of 28 K. M. from the Atlantic, no difficulties will be met with on account of the velocity of the current.

Finally as to the middle part of the canal extending for about 36 K. M. between K. M. 28 and K. M. 64 from the Atlantic.

For this part the *differences* between the velocities of the current, occurring therein, with those occurring in the preceding 28 K. M., may be computed with sufficient accuracy by means of the equation of continuity.

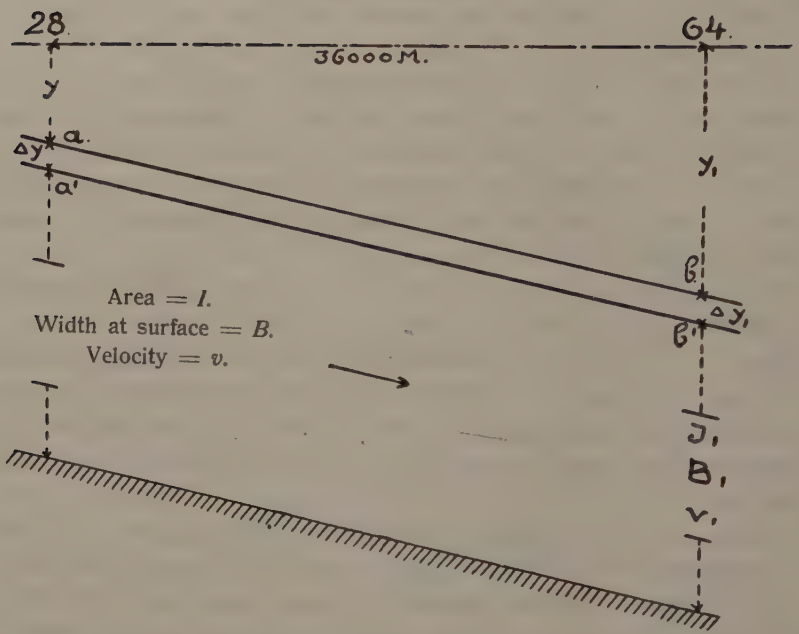
For, let ab be the canal's surface for this part, at the epoch t, a little before low water, at the distance of 64 K. M. from the Atlantic. Similarly let $a'b'$ be the canal's surface a second later, then necessarily

$$-\frac{(B+B_1)}{2} \times 36000 \times \frac{\Delta y + \Delta y_1}{2} = Iv - I_1 v_1$$

from which :

$$(v_1 - v) = \frac{36000\,(B + B_1)\,(\Delta y + \Delta y_1)}{4\,I_1} + v\,\frac{I - I_1}{I_1}$$

Now the quantities $I, I_1, B, B_1, \Delta y$ and Δy_1, are known for the epoch t, at least if we admit that — as is the case on the Suez-canal — the high- and lowwaterlines for the part 28—64 K.M. are



nearly straight lines, and further that the velocity of propagation of the tides is known with sufficient accuracy, likewise owing to observations made on the Suez-canal. Therefore we will be able to determine the difference of the velocities at 64 and 28 K.M. distance from the Atlantic, for the epochs at which $\frac{I-I_1}{I_1}$ is a small quantity. This will be the case near the moment of low water.

For the difference of the velocities v_1 and v, during the half hour preceding the moment of lowwater at K.M. 64, during which half hour the velocity of the current will be maximum at that point, we find as follows for spring-tide. We assume that between the distances 28 K.M. and 64 K.M. (from the Atlantic) there is a retardation of the tides of just one hour:

at $^1/_2$ hour before lowwater: $(v_1 - v) = 0.32$ M. $+ 0.02\, v$

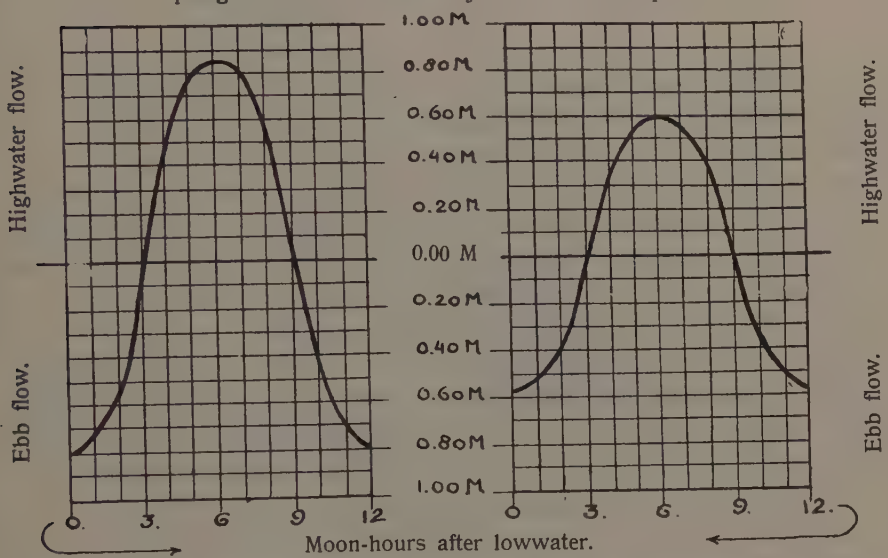
„ lowwater: $(v_1 - v) = 0.12$ M. $+ 0.015\, v$.

From these figures it appears, that during the half hour before lowwater at K.M. 64 the *differences* of the velocities of the current are only to a small extent dependent on the value of the velocity v. These differences, therefore, may be determined with sufficient precision, even if the velocity v is only approximatively known.

By observations made on the Suez-canal during the period 1871—

1876 the velocity-curve for a place near the Red Sea is known both for springtide and for neaptide. It has been represented in the following figures. [1])

Mean velocity-curves at the entrance of the Suez-canal.



The above velocity curves probably do not represent the mean velocities but the velocities in the middle of the canal. They have been derived from measurements made every hour partly by means of floats partly by means of the current meter of WOLTMANN.

It deserves attention, however, that at the time of these observations the Suez-canal had still only a depth of 8 M. below low water and a bottomwidth of 22 M. The section of the canal is now being increased to a bottomwidth of 45 M. and a depth of 10,5 M. below low water. The velocities in the widened canal may perhaps exceed by 20 percent those observed on the canal during the period 1871—1876.[2])

---

[1]) These curves are borrowed from the Etude du régime de la Marée dans le canal du Suez par M. BOURDELLES, in the Annales des Ponts et Chaussées of 1898. They occur originally in a Note sur le régime des eaux dans le canal maritime de Suez et à ses embouchures in 1884 by LEMASSON Chief Engineer of the canal-works.

[2]) For the original cross section of 8 M. depth below low water, 22 M. bottom-width and slopes of 1 vertical on 2 horizontal we have:

Area $I = 304$ M²; wet circumference $O = 57.9$ M., consequently $R = 5.25$ M. For the future cross section of 10.5 M. depth, 45 M. bottomwidth and slopes of 1 vertical on $2\frac{1}{2}$ horizontal, we will have: $I = 749$ M²., $O = 101.5$ M. therefore $R = 7.37$ M. Now $\sqrt{\frac{7.37}{5.25}} = 1.20$.

If, in consideration of this fact, we substitute in the second member of the formula, for v the values observed in the period 1871—1876 increased by 20 percent, we finally find

$^1/_2$ hour before low water $(v_1 - v) = 0.33$ M.
at „ „ $(v_1 - v) = 0.13$ M.

The differences 0.33 and 0.13 M. represent the differences of the simultaneous velocities, not those of the maximum velocities at the distances of 64 and 28 K.M. from the Atlantic.

At the moment that the velocity reaches its maximum at K.M. 64, the velocity at K.M. 28, where the tides set in about an hour later, will still be below the maximum at that place. According to the observations on the Suez-canal we may assume that, at the epochs mentioned, the velocities of the current at K.M. 28 will at least be about 0.15 M. and 0.05 M. below the maximum of that place. Hence we may conclude that the maximum velocities at K.M. 64 and 28 will certainly not differ 0.18 M, and probably not much over 0.08 M. We are sufficiently justified therefore in assuming that the velocity at K.M. 64 may be about 0.15 M. in excess of that of K.M. 28.

As appears from what has been stated before the difference is inferior to the increase of the velocity of the current on the Suez-canal under the influence of the wind, which may amount to 0.30—0.50 M. It cannot, therefore, cause any serious difficulty.

§ 7. For an open Panama-canal executed as follows:

From the Atlantic to K. M. 64 having the same normal cross section as that of the project for the sea-level canal;

from K. M. 64 to K. M. 68, which is the place where the canal will be connected with a lake gradually widening;

from K. M. 68 to the Pacific at K. M. 80 having bottomwidth of 400 to 500 feet;

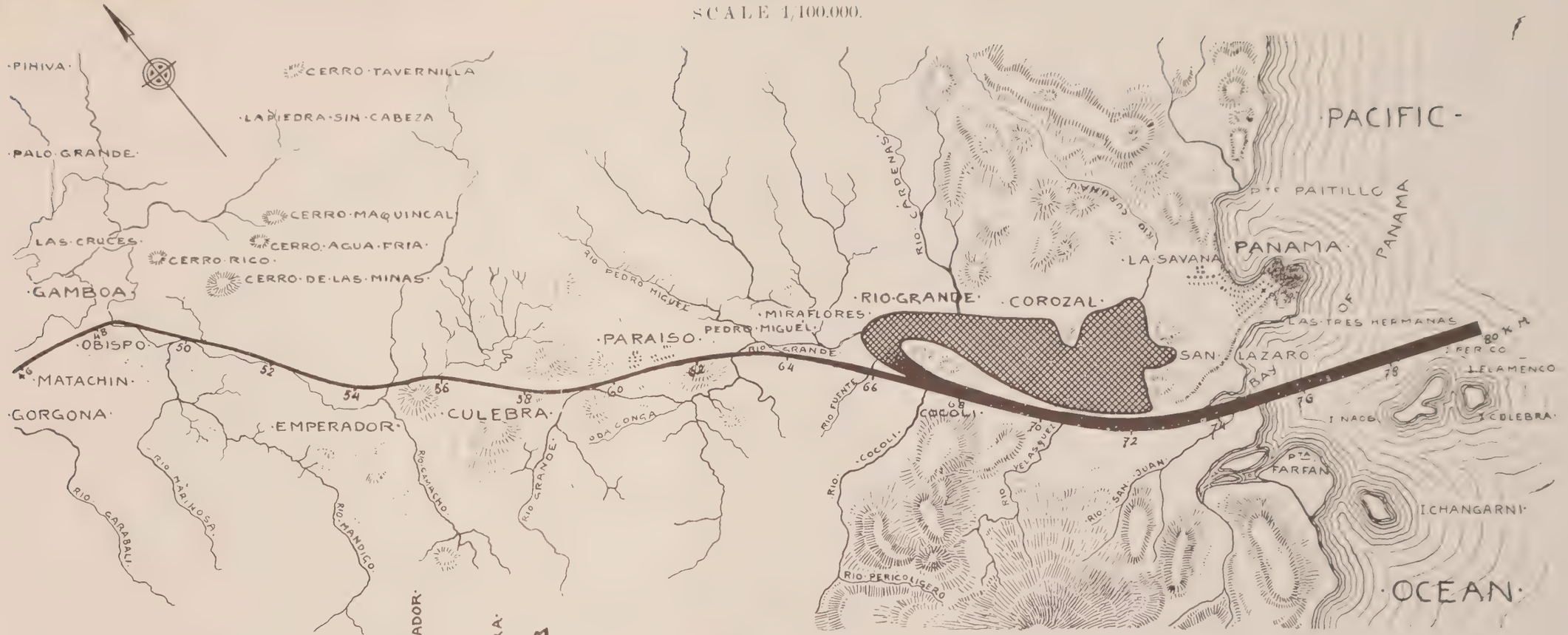
the following conclusions in regard to the velocities of the current at springtide may be accepted:

On the first 28 K. M. of such an open canal, velocities of the current will occur at springtide which, on an average, will be about equal to those, which will take place at spring tide and with a moderate wind on the Suez-canal between the Bitter Lakes and the Red Sea as soon as the widening of this canal will be complete.

On the subsequent 36 K. M. of such an open canal the maximum velocities at springtide will exceed those on the preceding part by

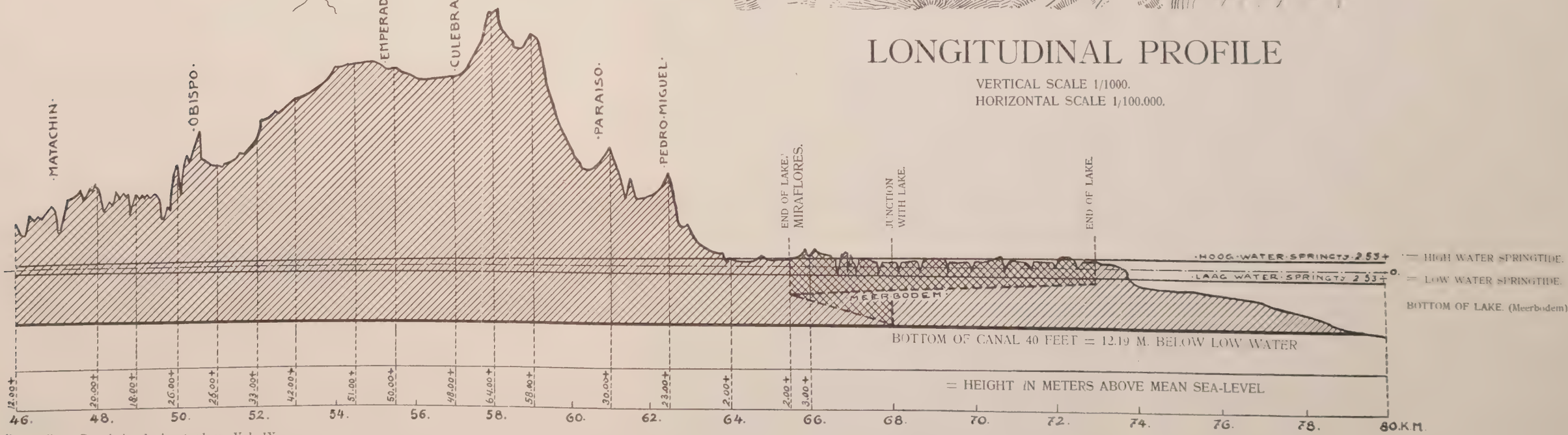
MAP.

SCALE 1/100.000.



LONGITUDINAL PROFILE

VERTICAL SCALE 1/1000.
HORIZONTAL SCALE 1/100.000.



= HIGH WATER SPRINGTIDE.
= LOW WATER SPRINGTIDE.
BOTTOM OF LAKE. (Meerbodem).

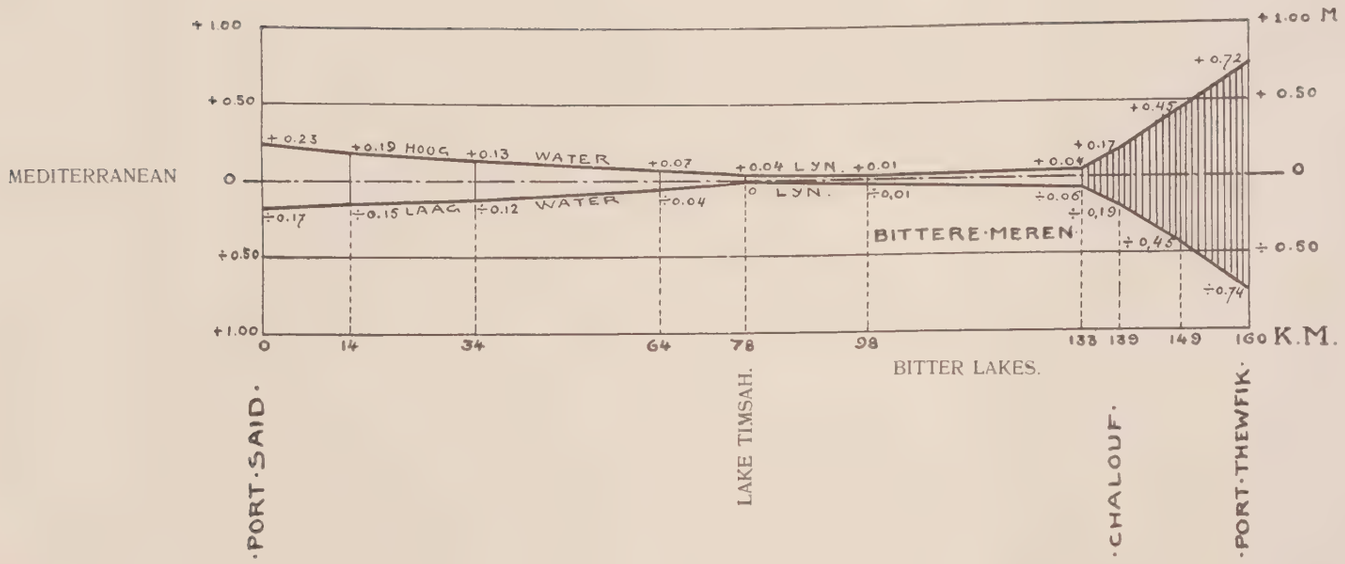
HIGH AND LOW WATER LINES

AT

SPRING TIDE.

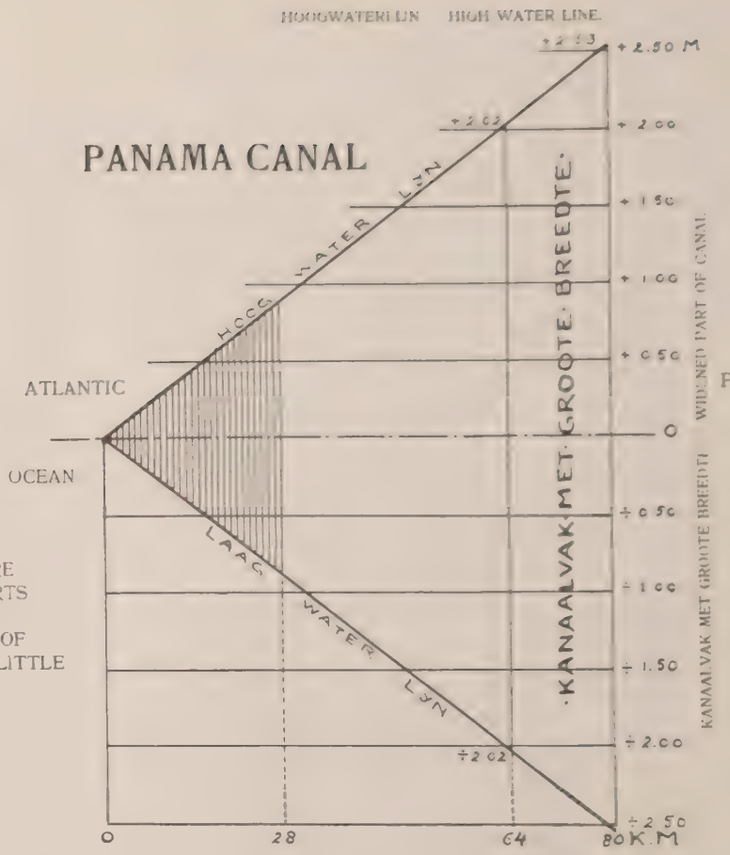
SUEZ-CANAL

(1871—1876)



THE HATCHED AREAS ARE RELATIVE TO THOSE PARTS OF THE TWO CANALS IN WHICH THE VELOCITIES OF THE CURRENT WILL BE LITTLE DIFFERENT.

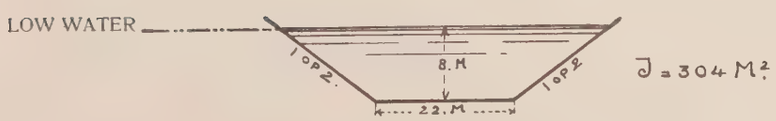
PANAMA CANAL



PROFILES

SUEZ-CANAL

A. AS ORIGINALLY EXECUTED.



B. IN EXECUTION.

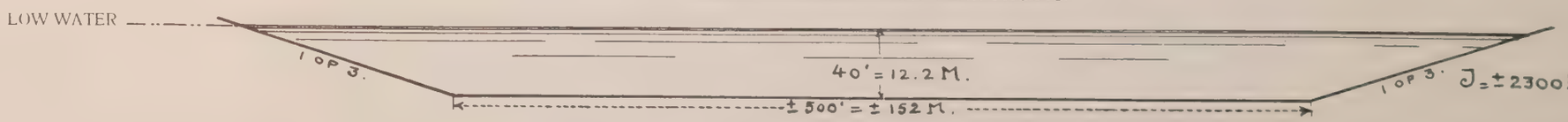
SCALE OF THE PROFILES 1/1000.

PANAMA-CANAL

A. IN LOW COUNTRY

·B·IN·CULEBRA·

C. GREATLY WIDENED PART NEAR PACIFIC.



about 0.15 M. They will not exceed however those on the Suez-canal with a strong wind.

For the last 16 K. M. of such an open canal the maximum velocities at springtide may be somewhat more considerable. On account however of the great width, which may be given to this part they will cause no serious difficulty.

Therefore, if we assume, as we have good reason to do, that even at spring tide and with wind the velocities of the current on the Suez-canal offer no serious difficulty to navigation we may conclude that on a Panama-canal of the above description also navigation will experience no difficulties on account of the velocities of the current.

Therefore, *if we leave out of consideration* the question whether an open Panama-canal without tidal lock is to be preferred either to a sea-level canal with such a lock, as proposed by the Board of Consulting Engineers, or to a summit level canal with three locks, as is now in course of execution, we may conclude, in the main in conformity with the conclusion of the French Academy of Sciences of 1887, but for different reasons:

That the velocities of the current due to tidal motion in an open Panama-canal without tidal lock will be no obstruction to navigation.

Zoology. — "*On the formation of red blood-corpuscles in the placenta of the flying maki* (*Galeopithecus*). By Prof. A. A. W. HUBRECHT.

(Communicated in the meeting of March 30, 1907).

At the meeting of November 26, 1898, I made a communication on the formation of blood in the placenta of Tarsius and other mammals, which was later completed by a more extensive paper, containing many illustrations (Ueber die Entwicklung der Placenta von Tarsius und Tupaja, nebst Bemerkungen über deren Bedeutung als hämatopoietische Organe; Report 4th Intern. Congress of Zoology, Cambridge 1898). The facts observed by me and the interpretation founded on them, have not until now been generally accepted, and in a recent very extensive discussion of the position of the problem concerning the origin of the red blood-corpuscles in the 14th volume of the "Ergebnisse der Anatomie und Entwicklungsgeschichte" (Wiesbaden 1905), by F. WEIDENREICH, the author, when mentioning my views, emits the supposition that I mixed up phagocytic and haematopoietic processes.

This conclusion was not based on a renewed and critical examination of the material, studied by me. I have regretted this, since I have pointed out clearly and repeatedly that the numerous prepara-

tions at Utrecht concerning this and other embryological problems are always available for comparative and critical work, also for those who do not share my views. Moreover it appears from the literature, mentioned in WEIDENREICH's paper, that the more extensive and illustrated article, quoted above, has remained unknown to him.

All this would not have induced me to return to this subject once more, were it not for the fact that during the last months I have become acquainted with the placentation-phenomena of a totally different mammal in which these phenomena have never yet been studied, namely Galeopithecus volans, which, like Tarsius, Nycticebus, Tupaja and Manis, was collected by me in the Indian Archipelago in 1890—1891 as extensively as possible for embryological purposes. During the first origin of the placenta of this rare and in many respects primitive mammal [1]), phenomena are observed which elucidate the process of blood-formation in the placenta in such an uncommonly clear manner that in this case it will be difficult to deny the evidence.

The formation of blood in the placenta of Galeopithecus may be said to take place according to a much simpler plan than in Tarsius, although the principal outlines remain the same and here also the non-nucleate haemoglobine-carrying blood-corpuscles must be regarded not as modified cells but as nuclear derivatives. Likewise the placenta of Galeopithecus bears testimony that not only the maternal mucosa but also the embryonic trophoblast takes part in the blood-formation, while the thus formed blood-corpuscles — also those that are furnished by embryonic tissue — circulate in the maternal blood-vessels only.

In Galeopithecus the process is simpler especially in this respect that here no megalokaryocytes play a part in the formation of blood, so that it is less easy — as WEIDENREICH did — to regard blood-corpuscles that are set free (such as we notice it in Tarsius, when the big lobed nuclei of these megalokaryocytes disintegrate) as being on the contrary devoured in that moment by phagocytosis! [2])

The haematopoiesis is started in Galeopithecus in the following manner. At about the same time that the young germinal vesicle, which has just gone through the two-layered gastrulation stage (gastrulation by delamination [3])), has attached itself to the surface of the strongly folded and swollen maternal mucous membrane, this mucous

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[1]) W. LECHE is inclined (Ueber die Säugethiergattung Galeopithecus, Svenska Akad. Handl. Bd. 21, N°. 11, 1886) to see in Galeopithecus a form which must be placed in the neighbourhood of the ancestral form of the bat.

[2]) Sectional series of Tarsius of a later date give a still clearer image than those which served for my figures of 1898.

[3]) See on this point Anatomischer Anzeiger Bd. 26, 353.

membrane reacts in the manner, well-known in other mammals (Tarsius, hedgehog, rabbit, bat,. etc.) by perceptible changes in the uterine glands in the vicinity of this place of attachment and by the formation of so-called trophospongia-tissue, consisting of a modification of the interglandular connective tissue, to which are added proliferations of uterine and glandular epithelium.

As the final product of these preliminary phenomena we now see that a part of the maternal mucosa where the germinal vesicle has coalesced with the mucosa, presents a more compact proliferation, while nearer the periphery the uterine glands, by strong dilatation of their lumen, differ clearly from the other uterine glands, as this is also the case in Tarsius, Lepus and other mammals during early pregnancy. The dilated glands may be followed up to their mouth; this mouth, however, no longer connects the glandular lumen with the uterine lumen, since in this place the embryonic trophoblast has disturbed the connection and covers the mouths of the glands.

This trophoblast now also shows unmistakable signs of cell-proliferation, although it does not at once attack and destroy the maternal epithelium, as in the hedgehog, Tarsius, Tupaja, etc. but rather finds itself facing this maternal epithelium in full proliferation, in the manner stated by me also for Sorex [1]). Instead of being closely adjacent, however, spaces are left open from the beginning between trophoblast and trophospongia, which spaces are partly mutually connected and partly are subdivided into smaller compartments by trophoblastic villi, attaching themselves to the trophospongia-tissue.

In this manner the free surface of the trophoblast, facing the embryo, obtains a knobbed appearance. [2])

Already in early developmental stages, when there is as yet no question of the folding off of the embryo and long before blood-carrying allantoic villi have become interlocked with these trophoblastic villi for the further completion of the placenta, we find in the spaces between trophoblast and trophospongia numerous blood-corpuscles of which we can not say that they have been carried thither by maternal vessels exclusively, although there can be no doubt that a connection between these spaces and the maternal vascular system is established at an early date. In the manner, indicated above, these spaces communicate also with the uterine glands which are here dilated. And in these glands as well as in

[1]) Quarterly Journal of Microscopical Science, vol. 35.

[2]) Certain modifications which I observed when the germinal vesicle develops in a uterus which is still in the puerperal stage, may be left out of account here.

the interglandular tissue and in the cells, lining the just mentioned spaces, phenomena take place which force us to the conclusion *that a great number of these blood-corpuscles originate in loco.* When we follow these phenomena up to their earliest appearance, we find that in the dilated glands in many places compact cell-heaps are formed, which sometimes lie quite loose in the gland, but in other cases are still found in direct connection with the cell-lining of the gland. We must assume that this latter condition represents the original one and that consequently we have here an epithelial proliferation by which new cell-material is carried into the region of the future placenta.

The final product of these lumps of tissue, which in early stages appear so distinctly as cell-heaps, is an agglomerate of non-nucleated blood-corpuscles. The gradual transition of the nucleate cells into the blood-discs may be followed step by step by successively comparing preparations of the youngest and subsequent stages: often in one preparation all transitions are found together. It then appears that the conclusions I drew for Tarsius and Tupaja in 1898 are confirmed here, viz. that the blood-discs are produced by gradual transitions from the modified nuclei of the above-mentioned cell-heaps and that in this process transitional stages are generally found, comparable to what I called "haematogonia" in the above-quoted paper. They resemble polynuclear leucocytes from which they may be distinguished, however (also according to MAXIMOW and SIEGENBEEK VAN HEUKELOM; see report of the meeting of the Amsterdam Academy of Nov. 26, 1898), by certain characteristics. This phenomenon has been more fully investigated by POLJAKOFF, who also regards the non-nucleate corpuscles as nuclear derivatives and not as cells, deprived of their nuclei. In his paper [1]) numerous illustrations are given of stages corresponding to my haematogonia. It appears from the literature, mentioned by POLJAKOFF that my paper of 1898, preceding his publication, was unknown to him: the concordant results which we have obtained at an earlier date, are confirmed in a striking manner by the phenomena seen in Galeopithecus.

But blood-corpuscles are also produced by other sources besides these epithelial glandular proliferations. Between the dilated glands we find in Galeopithecus in the trophospongia-tissue very conspicuous groups of large cells with a big, but circular nucleus. They show a tendency to lie together in nests, which nests are more or less kept together by elongated cells, forming a spurious wall which distantly remind us of an endothelium.

[1]) Biologie der Zelle. In Arch. f. Anat. u. Phys. Abth. 1901. Pl. I and II.

These cells also are gradually dissolved into blood-corpuscles: as the uterus grows and the trophospongia passes through its successive developmental stages, they disappear: the blood-corpuscles which owe their existence to them, fall into the above-mentioned spaces, from whence they are taken up in the further circulation. The intermediate stages that can be observed in this way of blood-formation, are in fact an increase of nuclei by amitosis, as was also described by POLJAKOFF and later a gradual formation from these nuclear derivates of non-nucleated blood-discs.

To these two processes of blood-formation in the placenta of Galeopithecus a third must be added in which not the mother is the active agent, as in the two former cases, but the embryonic trophoblast. Of this trophoblast we described above how it forms the bottom of the cavities into which the newly-formed blood-corpuscles are discharged, and how it coalesces with the maternal trophospongia to such an extent that for many cells, which here are closely adjacent, it is impossible to determine whether they take their origin in the mother or in the trophoblast of the germinal vesicle.

Yet in regard to the wall of the cavities, which separates them from the lumen of the uterus, there can be no doubt that we have here trophoblastic tissue only. About the active proliferation of this trophoblast tissue there is no doubt, no more than about the question whether the numerous parts of this trophoblast that project into the cavities, partake in the haematopoiesis. As soon as these parts are examined with strong powers it is quite evident that here the nuclei of the trophoblast cells undergo similar modifications as were described above and that the final product of these modifications are again red non-nucleated blood-corpuscles which are added to those already present and originating from the mother. Now these corpuscles are, in the same way as I observed ten years ago in Tarsius and Tupaja, *set free into the maternal circulation and carried along by it.*

On the theoretical significance of the fact that the germinal vesicle takes an active and important part in increasing the number of units for the transport of oxygen in the maternal blood, I will not expatiate here.

And for the histological details of the formation of the bloodplates, resp. non-nucleated blood-corpuscles from an originally normal cell-nucleus, I refer to the coloured figures of pl. I and II of POLJAKOFF's paper in the 1901 volume of the Arch. f. Anat. u. Phys. (Anat. Abth.). With his illustrations I can identify everything I have observed in Galeopithecus. While in a very few cases there seems to be a possibility that the blood-corpuscle owes its existence to a

change of the nucleus *in its entirety*, in the vast majority of cases a distinct amitotic disintegration is observed, the number of fragments varying, but generally lying between three and five. As the already modified nucleus dissolves into these fragments the comparability with polynuclear leucocytes seems more obvious, and the colour as a rule approaches more and more to that which the blood-corpuscles themselves assume in the artificially fixed preparation. The same fact was stated by me also for Tarsius in 1898 and figured on Pl. 14 figs. 91—96.

Finally I point out, since my results and those of Poljakoff agree in so many respects, that also Retterer in the volume for 1901 of the Journal de l'Anatomie et de la Physiologie (Structure, développement et fonction des ganglions lymphatiques, p. 700) has obtained similar results and is inclined to assume a still closer genetic relationship between polynuclear leucocytes and haematogonia when he declares that the leucocytes, liberated from lymphatic glands "finissent par se convertir, dans la lymphe ou le sang, en hématies grâce à la transformation hémoglobique *de leur noyau . . .*"

Thus my observations on Galeopitheous form a link in the chain, which begins with Heinrich Müller in 1845 (Zeitschrift für rationelle Medicin vol. 3. p. 260) was then continued and upheld by Wharton Jones (Phil. Trans. 1846, p. 65 and 71) and Huxley (Lessons in Elementary Physiology, 1866, p. 63) and which, since in 1898 Tarsius added another link, has with increasing weight bound up the question of the origin of the non-nucleated blood-corpuscles in mammals to the conception that these elements in the mammalian body are not equivalent with cells, but must be regarded as nuclear derivatives.

(May 24, 1907).

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